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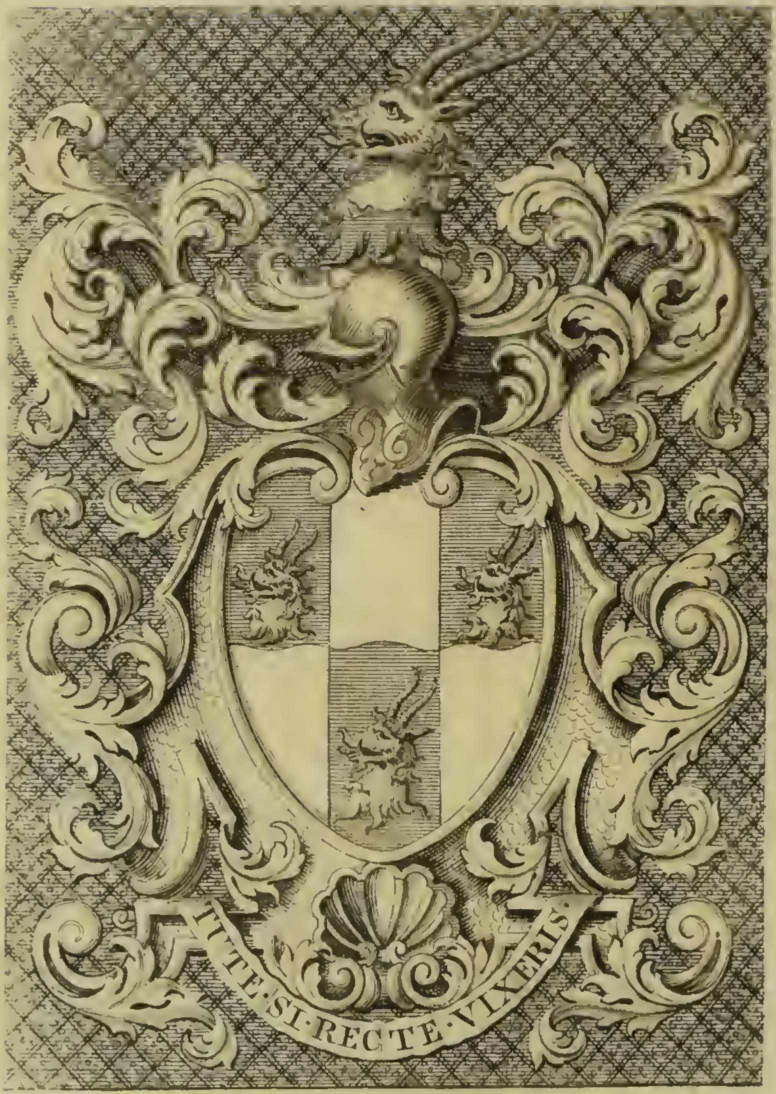
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PHILOSOPHICAL TRANSACTIONS, OF THE ROYAL SOCIETY OF LONDON.

VOL. LXXVIII. For the Year 1788.

PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the Year 1787, the Medal on Sir Godfrey Copley's Donation, to John Hunter, Eſq. F. R. S. for three Papers which were printed in the laſt Volume of the Philoſophical Tranſactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *Of the Methods of manifeſting the Preſence, and aſcertaining the Quality, of ſmall Quantities of Natural or Artificial Electricity. By Mr.* Tiberius Cavallo, *F. R. S.*

The Lecture founded by the late HENRY BAKER, Eſq. F. R. S.

Read November 15, 1787.

THE perſevering induſtry of many ingenious perſons in the philoſophical world, ſince the beginning of the preſent century, has gradually, and with rapid ſucceſs, examined the effects, aſcertained the properties, and inveſtigated the laws of a ſcience almoſt entirely unknown before. Various wonderful phænomena of nature have been explained, means have been diſcovered of avoiding ſome of their dangerous effects, and a greater inſight into the general operations of nature has been derived, from the knowledge of ſo great and ſo extenſive a power as is commonly underſtood under the name of *electricity*; yet, if inſtead of contemplating its more ſtriking phænomena, *viz.* thoſe which are apt to ſurpriſe more by the magnitude of their effects, than by the inſtruction which they may afford, we endeavour to examine the ſubject more cloſely, we ſhall find that our knowledge of electricity goes very little, if at all, beyond the ſuperficial part of it. We know, for inſtance, that a piece of glaſs, or other electric, when rubbed, will produce that power which we call electricity, that the glaſs will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumſtances, and ſo on; but no perſon has ſhewn how that power is generated by the friction, or what prevents its paſſage through the ſubſtance of ſome particular bodies. It has been aſcertained, that the air of moſt countries, and probably of the whole world, as well as the clouds, fogs, rains, &c. are almoſt always electrified; but we are ignorant of the office which this electricity can have in the great laboratory of nature; for ſurely ſo general and ſo active a power can hardly be intended by nature, merely to intimidate mankind now and then with the thunder and the lightning.

It appears, therefore, that thoſe perſons, who are now willing to diſtinguiſh themſelves in this intereſting branch of natural philoſophy, ought to examine the electrical power not ſo much in its accumulated, as in its incipient ſtate. Its firſt origin, or very beginning, ought to be inveſtigated, it being afterwards very eaſy to underſtand its increaſe; for we may eaſily comprehend, how a great quantity of it may be accumulated from the often repeated additions of its ſmalleſt portions.

This truth having been underſtood by ſeveral philoſophical perſons, has induced them to contrive inſtruments neceſſary for the purpoſe, and to make numerous obſervations, which have undoubtedly promoted the knowledge of the ſubject; but a great deal ſtill remains to be done, before we can attain to the knowledge of the object in view, *viz.* of the real nature, of the firſt origin, and of the general uſe of electricity. The inſtruments hitherto invented are ſtill inadequate to the purpoſe, and the known methods of operating are not free from conſiderable objections. To examine the peculiar conſtructions, intended uſes, properties, and defects, of thoſe inſtruments, as well as methods of performing the experiments, is the principal object of the preſent Lecture, which I have the honour of delivering to this learned Society, and which may, perhaps, be of uſe to other experimenters in this branch of philoſophy; ſince what is mentioned in the following pages is the reſult of long experience, and of conſideration. Of experience, which has often contradicted the ſanguine expectations of a pre-conceived theory; and of conſideration, which has frequently demonſtrated the abſurdity or inſufficiency of ſchemes which, at firſt ſight, ſeemed very eaſy and plauſible.

The late Mr. JOHN CANTON was, as far as I know, the firſt perſon who conſtructed an electrometer, or inſtrument capable of ſhewing the preſence of what was then conſidered as a ſmall quantity of electricity. This inſtrument conſiſted of two ſmall balls of pith of elder or of cork, faſtened to the two extremities of a linen thread, the middle of which was faſtened to an oblong wooden box, in which the thread and balls were kept, when not actually in uſe. It was with ſuch an inſtrument that Mr. CANTON himſelf, Father BECCARIA, and others, aſcertained the electricity generally exiſting in the air; and that Mr. RONAYNE diſcovered the conſtant electricity of fogs. But in the courſe of my experiments, having made frequent uſe of ſuch electrometers, it naturally occurred to me, that in ſeveral caſes, when the electrometer gave no ſigns of electricity, or at leaſt not ſufficient to aſcertain its quality, the cauſe of it was the reſpectively large ſize of the inſtrument; for a ſmall quantity of electricity being diffuſed through the box, thread, and balls of the electrometer, had not power ſufficient to ſeparate the balls, and of courſe to ſhew its preſence. In conſequence of this, I contracted the ſize of the electrometers to ſuch a degree as could be affected by leſs than the tenth part of that quantity of electricity which was neceſſary to affect Mr. CANTON's electrometer. But in making the electrometers very ſhort, the ſtiffneſs of the threads, which had been inſignificant in a great extent, became now very conſiderable; hence, inſtead of faſtening the balls to the two extremities of one piece of thread, I found it neceſſary to ſuſpend each ball by a ſeparate piece of thread, the upper part of which was formed into a loop, which moved in a ring of braſs wire.

The electrometers, thus improved, were ſtill ſubject to a very great imperfection, which was the twiſting of the threads; to avoid which I ſubſtituted fine ſilver wire, inſtead of linen threads, which anſwered very well. However, in obſerving the electricity of the atmoſphere, thoſe electrometers appeared to labour under a conſiderable inconvenience, which was their being diſturbed by the wind; for it frequently happened, that neither the exiſtence, nor the quality, of the electricity could be aſcertained on account of the wind agitating the inſtrument, and becauſe the electricity being in ſmall quantity would not remain on the electrometer a time ſufficient to bring the inſtrument within doors, where it might be examined without any obſtruction. In order to remove this imperfection, I incloſed the electrometer in a bottle, as it is deſcribed in the Philoſophical Tranſactions, Vol. LXX. which conſtruction has been found to anſwer remarkably well.

This bottle electrometer has been ſince altered by various perſons; though, in my opinion, thoſe alterations do not tend to improve it altogether. M. DE SAUSSURE, by altering the ſhape of the bottle, and depriving it of a neck, has rendered it capable of retaining the communicated electricity only for a very ſhort time*; whereas, ſome of thoſe electrometers, conſtructed on my original plan, have retained the communicated electricity for more than four hours.

Beſides this, M. DE SAUSSURE, as well as ſome who make theſe electrometers for ſale, have ſubſtituted pith balls, inſtead of the conical corks of the original plan, the latter of which are preferable for two reaſons; *viz.* firſt, becauſe the balls of pith are apt to adhere to each other, ſo that on communicating the electricity they often do not ſhew any repulſion at all; or if the communicated electricity be conſiderably great, the pith balls, after adhering to each other for ſome time, are at laſt ſeparated with violence, ſo as to fly to the ſides of the bottle, which fruſtrates the experiment; whereas the corks are not near ſo ſubject to this inconvenience; and, ſecondly, becauſe the conical ſhape renders thoſe corks capable of preſenting a much greater ſurface to each other, than if they were globular, their weights being the ſame in both caſes; for it is very well known, that, of all the ſolid figures, the ſphere contains the greateſt capacity under the leaſt ſurface; and as the effects of electricity, in thoſe caſes, is proportionate to the ſurface, and not to the quantity of matter, it follows, that the conical ſhape is preferable to the globular.

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* SAUSSURE's Voyage dans les Alpes, Tom. II.

Another alteration of the bottle electrometer was lately made by the Rev. Mr. BENNET, and is deſcribed in the Philoſophical Tranſactions, Vol. LXXVII. Part I. It conſiſts principally in ſubſtituting two ſlips of gold-leaf to the corks ſuſpended by wires. This alteration has ſome peculiar advantages and diſadvantages. Its advantages are in general a greater degree of ſenſibility, and a more eaſy conſtruction. Its diſadvantages are, firſt, that the inſtrument is not portable; and, ſecondly, that even when not carried about, it is apt to be ſpoiled very eaſily. However, in ſome caſes it is very uſeful, ſo that, upon the whole, it may be conſidered as a very good improvement.

As the faſtening of the ſlips of gold-leaf to this electrometer, and to let them hang parallel, is rather difficult, it will not be improper to deſcribe a method which I have contrived, in order to accompliſh it more eaſily. When the ſlips are cut, and are lying upon paper or leather, I make them equal in length, by meaſuring them with a pair of compaſſes, and cutting off a portion from the longeſt: then cut two bits of very fine paper, each about half an inch long, and a quarter of an inch broad, and ſtick one of them, by means of a little wax, which is rubbed upon them, to one extremity of each ſlip of gold-leaf, ſo as to form a kind of letter T. This done, I hold up, in the fingers of one hand, one piece of paper, with the gold leaf ſuſpended to it, and hold the other with the other hand;

then bring them near to each other, and after having adjuſted them properly, ſo that the ſlips may hang parallel and ſmooth, I force the pieces of paper, which now touch each other, between the two ſides of a ſort of pincers made of braſs wire, which are faſtened to the under part of that piece which forms the top of the electrometer: and, in order to change thoſe ſlips when ſpoiled, I keep in a book ſeveral ſlips of gold-leaf ready cut, and furniſhed each with a piece of paper; ſo that by this means this electrometer is rendered, in a certain manner, portable.

Beſides the way of aſcertaining ſmall quantities of electricity, by means of very delicate electrometers, two methods have been communicated to the philoſophical world, by which ſuch quantities of electricity may be rendered manifeſt, as could not be perceived by other means. The firſt of thoſe methods is an invention of M. VOLTA, the apparatus for it being called the *Condenſer of Electricity,* and is deſcribed in the Philoſophical Tranſactions, Vol. LXXII. The ſecond is a contrivance of the above-mentioned Mr. BENNET, who calls the apparatus *The Doubler of Electricity.* A deſcription of it is inſerted in the Philoſophical Tranſactions, Vol. LXXVII.

M. VOLTA's condenſer conſiſts of a flat and ſmooth metal plate, furniſhed with an inſulating handle, and a ſemi-conducting, or imperfectly inſulating, plane. When one wiſhes to examine a weak electricity with this apparatus, as that of the air in calm and hot weather, which is not generally ſenſible to an electrometer, he muſt place the above-mentioned plate upon the ſemi-conducting plane, and a wire, or ſome other conducting ſubſtance, muſt be connected with the metal plate, and muſt be extended in the open air, ſo as to abſorb its electricity; then, after a certain time, the metal plate muſt be ſeparated from the ſemi-conducting plane, and being preſented to an electrometer will electrify it much more than if it had not been placed upon the above-mentioned plane.

The principle on which the action of this apparatus depends is, that the metal plate, whilſt ſtanding contiguous to the ſemi-conducting plane, will both abſorb and retain a much greater quantity of electricity than it can either abſorb or retain when ſeparate, its capacity being increaſed in the former, and diminiſhed in the latter caſe.

Whoever conſiders this apparatus, will eaſily find, that its office is not to manifeſt a ſmall quantity of electricity, but to condenſe an expanded quantity of electricity into a ſmall ſpace; hence, if by means of this apparatus one expected to render more manifeſt than it generally is, when communicated immediately to an electrometer, the electricity of a ſmall tourmalin, or of a hair when rubbed, he would find himſelf miſtaken.

It is Mr. BENNET's doubler that was intended to anſwer that end; *viz.* to multiply, by repeated doubling, a ſmall, and otherwiſe unperceivable, quantity of electricity, till it became ſufficient to affect an electrometer, to give ſparks, &c. The merit of this invention is certainly conſiderable; but the uſe of it is far from preciſe and certain.

This apparatus conſiſts of three braſs plates, which we ſhall call A, B, and C; each of which is about three or four inches in diameter. The firſt plate A is placed upon the gold-leaf electrometer, or it may be ſupported horizontally by any other inſulating ſtand, and its upper part only is thinly varniſhed. The ſecond plate B is varniſhed on both ſides, and is furniſhed with an inſulating handle, which is faſtened laterally to the edge of it. The third plate C is varniſhed on the under ſide only, and is furniſhed with an inſulating handle, which is perpendicular to its upper ſurface.

This apparatus is uſed in the following manner. The plate B being laid upon the plate A, the ſmall quantity of electricity, which is required to be multiplied, is communicated to the under part of the plate A, and at the ſame time the upper part of B is touched with a finger; then the finger is firſt removed; the plate B is afterwards removed from over the plate A. The plate C is now laid upon B, and its upper ſurface is touched, for a ſhort time, with a finger. By this operation it is clear, that if the electricity communicated to the plate A is poſitive, the plate B muſt have acquired a negative electricity, and the plate C muſt have acquired the poſitive, *viz.* the ſame of the plate A. Now the plate B, being ſeparated from C, is laid as before upon A; the edge of C is brought into contact with the under part of the plate A, and at the ſame time the upper part of B is touched with a finger, by which means the plate B, being acted upon by the atmoſpheres of both the plates A and C, will acquire nearly twice as much electricity as it did the firſt time, and of courſe will render the plate C, when that is laid upon it, proportionably more electrified than before: thus, by repeating this operation, the electricity may be increaſed to any required degree.

The varniſh on thoſe ſurfaces of the plates which are to lie contiguous to each other ſerves to prevent the metal of one touching the metal of the other; for in that caſe, inſtead of one plate cauſing a contrary electricity in the other, the electricity of the firſt would be gradually communicated to the others, and would be diſſipated.

As ſoon as I underſtood the principle of this contrivance, I haſtened to conſtruct ſuch an apparatus, in order to try ſeveral experiments of a very delicate nature, eſpecially on animal bodies and vegetables, which could not have been attempted before, for want of a method of aſcertaining exceedingly ſmall quantities of electricity; but, after a great deal of trouble, and many experiments, I was at laſt forced to conclude, that the doubler of electricity is not an inſtrument to be depended upon, for this principal reaſon; *viz.* becauſe it multiplies not only the electricity which is willingly communicated to it from the ſubſtance in queſtion; but it multiplies alſo that electricity which in the courſe of the operation is almoſt unavoidably produced by accidental friction; or that quantity of electricity, however ſmall it may be, which adheres to the plates in ſpite of every care and precaution.

Having found, that with a doubler conſtructed in the above deſcribed manner, after doubling or multiplying twenty or thirty times, it always became ſtrongly electrified, though no electricity had been communicated to it before the operation, and though every endeavour of depriving it of any adhering electricity had been practiſed; I naturally attributed that electricity which appeared after repeatedly doubling, to ſome friction given to the varniſh of the plates in the courſe of the operation. In order to avoid entirely this ſource of miſtake, or at leaſt of ſuſpicion, I conſtructed three plates without the leaſt varniſh, and which, of courſe, could not touch each other, but were to ſtand only within about one-eighth of an inch of each other. To effect this, each plate ſtood vertical, and was ſupported by two glaſs ſticks, which were covered with ſealing-wax: ſee Tab. I. fig. 1. and 2., where AB is a wooden pedeſtal, 7½ inches long, 2½ broad, and 1¼ inch thick; C and D are the two glaſs ſticks cemented into the ſtand or pedeſtal AB, and likewiſe into the piece of wood E, which is faſtened to the back of the plate. The plate itſelf is of ſtrong tin, and meaſures about eight inches in diameter. The ſtand AB projects very little before the plate, by which means, when two of thoſe plates are placed upon a table facing each other, the wooden ſtands will prevent their coming into actual contact, as may be clearly perceived in fig. 3.

I need not deſcribe the manner of doubling or of multiplying with thoſe plates; the operation being eſſentially the ſame as when the plates are conſtructed according to Mr. Bennet's original plan, excepting that, inſtead of placing them one upon the other, mine are placed facing each other; and in performing the operation they are laid hold of by the wooden ſtand AB; ſo that no friction can take place either upon the glaſs legs, or upon any varniſh; for theſe plates have no need of being varniſhed. Sometimes, inſtead of touching the plates themſelves with the finger, I have fixed a piece of thin wire to the back of the plate, and have then applied the finger to the extremity of the wire, ſuſpecting that ſome friction and ſome electricity might poſſibly be produced when the finger was applied in full contact to the plate itſelf.

It is evident, that as the plates do not come ſo near to each other in this, as they do in the other conſtruction, the electricity of one of them cannot produce ſo great a quantity of the contrary electricity in the oppoſite plate; hence, in this conſtruction, it will be neceſſary to continue the operation of doubling ſomewhat longer; but this diſadvantage is more than repaid by the certainty of avoiding any friction.

Having conſtructed thoſe plates, I thought that I might proceed to perform the intended experiments without any further obſtruction; but in this I found myſelf quite miſtaken: for, on trying to multiply with thoſe new plates, and when no electricity had been previouſly communicated to any of them, I found, that after doubling ten, fifteen, or at moſt twenty times, they became ſo full of electricity as to afford even ſparks. All my endeavours to deprive them of electricity proved ineffectual. Neither expoſing them, and eſpecially the glaſs ſticks, to the flame of burning paper, nor breathing upon them repeatedly, nor leaving them untouched for ſeveral days, and even for a whole month, during which time the plates remained connected with the ground by means of good conductors, nor any other precaution I could think of, was found capable of depriving them of every veſtige of electricity; ſo that they might ſhew none after doubling ten, fifteen, or at moſt twenty times.

The electricity produced by them was not always of the ſame ſort; for ſometimes it was negative for two or three days together; at other times it was poſitive for two or three days more; and often it changed in every operation. This made me ſuſpect, that poſſibly the beginning of that electricity was derived from my body, and being communicated by the finger to the plate that was firſt touched, was afterwards multiplied. In order to clear this ſuſpicion, I actually tried thoſe plates at different times, *viz.* before and after having walked a great deal, before and after dinner, &c. noting very accurately the quality of the electricity produced each time; but the effects ſeemed to be quite unconnected with the above-mentioned concomitant circumſtances, which independence was further confirmed by obſerving that the electricity produced by the plates was of a fluctuating nature, even when, inſtead of touching the plates with the finger, they had been touched with a wire, which was connected with the ground, and which I managed by means of an inſulating handle.

At laſt, after a great variety of experiments, which it is unneceſſary to deſcribe, I became fully convinced, that thoſe plates did always retain a ſmall quantity of electricity, perhaps of that ſort with which they had been laſt electrified, and of which it was almoſt impoſſible to deprive them. The various quality of the electricity produced was owing to this, *viz.* that as one of thoſe plates was poſſeſſed of a ſmall quantity of poſitive electricity, and another was poſſeſſed of the negative electricity, that plate which happened to be the moſt powerful, occaſioned a contrary electricity in the other plate, and finally produced an accumulation of that particular ſort of electricity.

Thoſe obſervations evidently ſhew, that no preciſe reſult can be obtained from the uſe of thoſe plates, and of courſe that, when conſtructed according to the original plan, they are ſtill more equivocal, becauſe they admit of more ſources of miſtake.

As thoſe plates, after doubling or multiplying only four or five times, ſhew no ſigns of electricity, none having been communicated to them before, I imagined that they might be uſeful ſo far only, *viz.* that when a ſmall quantity of electricity is communicated to any of them in the courſe of ſome experiment, one might multiply it with ſafety four or five times, which would even be of advantage in various caſes; but in this alſo my expectations were diſappointed, as will appear from the following pages.

Having obſerved, after many experiments, that, *ceteris paribus*, when I began to multiply from a certain plate, for inſtance A, the electricity which reſulted was generally poſitive; and when I began with another plate B, *viz.* conſidered this plate B as the firſt plate, the reſulting electricity was generally negative; I communicated ſome negative electricity to the plate A, with a view of deſtroying its inherent poſitive electricity. This plate A being now electrified negatively, but ſo weakly as juſt to affect an electrometer, I began doubling; but after having doubled three or four times, I found, by the help of an electrometer, that the communicated negative electricity in the plate was diminiſhed inſtead of being increaſed; ſo that ſometimes it vaniſhed entirely, though by continuing the operation it often began to increaſe again, after a certain period. This ſhews, that the quantity of electricity, which however ſmall it may be, remains in a manner faſtened to the plates, will help either to increaſe or to diminiſh the accumulation or multiplication of the communicated electricity, according as it happens to be of the ſame, or of a different nature.

After all the above-mentioned experiments made with thoſe doubling or multiplying plates, we may come to the following concluſion, *viz.* that the invention is very ingenious, but their uſe is by no means to be depended upon. It is to be wiſhed, that they may be improved, ſo as to obviate the weighty objections that have been mentioned in the preceding pages, the firſt deſideratum being to conſtruct a ſet of ſuch plates as, when no electricity is communicated, they will produce none after having performed the operation of doubling for a certain number of times.

Upon the whole, the methods by which ſmall quantities of electricity may be aſcertained with preciſion are, as far as I know, only three. If the abſolute quantity of electricity be ſmall and pretty well condenſed, as that produced by a ſmall tourmalin when heated, or by a hair when rubbed, the only effectual method of manifeſting its preſence, and aſcertaining its quality, is to communicate it immediately to a very delicate electrometer, *viz.* a very light one, that has no great extent of metallic or of other conducting ſubſtance; becauſe if the ſmall quantity of electricity that is communicated to it be expanded throughout a proportionably great ſurface, its elaſticity, and of courſe its power of ſeparating the corks of an electrometer, will be diminiſhed in the ſame proportion.

The other caſe is, when one wants to aſcertain the preſence of a conſiderable quantity of electricity, which is diſperſed or expanded into a great ſpace, and is little condenſed, like the conſtant electricity of the atmoſphere in clear weather, or like the electricity which remains in a large Leyden phial after the firſt or ſecond diſcharge.

To effect this, I uſe an apparatus, which in principle is nothing more than Mr. VOLTA's condenſer; but with certain alterations, which render it leſs efficacious than in the original plan, but at the ſame time render it much leſs ſubject to equivocal reſults. I place two of the above deſcribed tin plates upon a table, facing each other, as ſhewn in fig. 3 and about one-eighth of an inch aſunder. One of thoſe plates, for inſtance A, is connected with the floor by means of a wire, and the other plate B is made to communicate, by any convenient means, with the electricity that is required to be collected. In this diſpoſition the plate B, on account of the proximity of the other plate, will imbibe more electricity than if it ſtood far from it, the plate A in this caſe acting like the ſemi-conducting plane of M. VOLTA's condenſer, though not with quite an equal effect, becauſe the other plate B does not touch it; but yet, for the very ſame reaſon, this method is incomparably leſs ſubject to any equivocal reſult. When the plates have remained in the ſaid ſituation for the time that may be judged neceſſary, the communication between the plate B, and the conducting ſubſtance which conveyed the electricity to it, muſt be diſcontinued by means of a glaſs ſtick, or other inſulating body; then the plate A is removed, and the plate B is preſented to an electrometer, in order to aſcertain the quality of the electricity; but if the electrometer be not affected by it, then the plate B is brought with its edge into contact with another very ſmall plate, which ſtands upon a ſemi-conducting plane, after the manner of M. VOLTA's condenſer*; which done, the ſmall plate, being held by its inſulating handle, is removed from the inferior plane, and is preſented to the electrometer: and it frequently happens, that the ſmall plate will affect the electrometer very ſenſibly, and quite ſufficient for the purpoſe; whereas the large plate itſelf ſhewed no clear ſigns of electricity.

If it be aſked, why I uſe the ſemi-conducting plane for this ſmall plate, and not for the large one? the anſwer is, firſt, becauſe the large ſemi-conducting plane is incomparably more difficult to be procured than the ſmall one; and, ſecondly, becauſe the ſmall plane may be eaſily deprived of any accidental electricity which may adhere to it; but the large one is more difficultly rendered fit for the purpoſe, eſpecially as the large plate ought in general to remain upon it a much longer time than the ſmall plate is to remain upon its ſemi-conducting plane.

The third and laſt caſe is when the electricity to be aſcertained is neither very conſiderable in quantity, nor much condenſed; ſuch is the electricity of the hair of certain animals, of the ſurface of chocolate when cooling, &c. In this caſe the beſt method is to apply a metal plate, furniſhed with an inſulating handle, like an electrophorus plate, to the electrified body, and to touch this plate with a finger for a ſhort time whilſt ſtanding in that ſituation; which done, the plate is removed, and is brought near an electrometer; or its electricity may be communicated to the plate of a ſmall condenſer, as directed in the preceding caſe, which will render the electricity more conſpicuous. It is evident, that in this caſe the metal plate will acquire the electricity contrary to that of the ſubſtance in queſtion; but this anſwers the ſame purpoſe: for if the electricity of the plate be found to be poſitive, one muſt conclude, that the electricity of the body in queſtion is negative, and contrariwiſe. In this operation, care muſt be had not to put the metal plate too near, or in full contact with the ſubſtance to be examined, leſt the friction, likely to happen between the plate and the body, ſhould produce ſome electricity, the origin of which might be attributed to other cauſes.

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* This ſmall plate is nearly of the ſize of a ſhilling, and the ſemi-conducting plane is of wood covered with copal varniſh.

Having thus far deſcribed the ſureſt methods of aſcertaining the preſence and quality of electricity, when its quantity or degree of condenſation is ſmall, I ſhall now beg leave to add ſome farther remarks on the ſubject of electricity in general, and which have been principally ſuggeſted by what has been mentioned in the preceding pages.

On the hypotheſis of a ſingle electric fluid, it is ſaid, that every ſubſtance in nature, when not electrified, contains its proper ſhare of electric fluid, which is proportionate to its bulk, or to ſome other of its properties; and it is generally believed, that this equal or proportionate diſtribution of electric fluid takes place with the greateſt part of natural bodies. However, the fact is far from being ſo; and I may venture to aſſert that, ſtrictly ſpeaking, every ſubſtance is always electrified, *viz.* that every ſubſtance, and even the various parts of the ſame body, contain at all times more or leſs electric fluid than that quantity of it which it ought to contain, in order to be in an electrical equilibrium with the bodies that ſurround it.

At firſt ſight it may be thought quite immaterial to know, whether the electric fluid is diſperſed in the juſt proportion among the various ſubſtances which are not looked upon as electrified, or whether it deviates in a ſmall degree from that proportionate diſtribution; but it will hereafter appear, that one of thoſe aſſertions will lead us to the explanation of an intereſting phænomenon in electricity, whereas the other does not admit of it; beſides, what is called ſmall difference of the proportionate diſtribution, inſomuch as it does not affect our inſtruments, may be ſufficient for ſeveral operations of nature, which it is our intereſt to inveſtigate.

If we inquire what phænomena evince this altered diſtribution, or the actually electrified ſtate of all bodies, the preceding obſervations will furniſh ſome very unequivocal ones; eſpecially that of the doubling plates made after my plan, which ſhewed to be electrified even after having remained untouched for a whole month, during which time they had been in communication with the ground; for if each of them had contained an equal ſhare of electric fluid, the electric atmoſphere of one of them could not poſſibly occaſion a contrary electricity in the other, and conſequently no accumulation of that power could have happened.

A great number of inſtances are related in books on the ſubject of electricity, and in the Philoſophical Tranſactions, of pieces of glaſs, of ſulphur, of ſealing-wax, &c. having remained electrified ſo far as to affect an electrometer for months after they had been excited, or even touched; but the following experiment will ſhew, in a clearer manner, the great length of time that a quantity of electricity will remain upon a body.

Having conſtructed a gold-leaf electrometer in the niceſt manner I could, and which, on account of the non-conducting nature and conſtruction of its upper part, could remain ſenſibly electrified for ſeveral hours together, I communicated ſome electricity to it, which cauſed the ſlips of gold-leaf to diverge with a certain angle; and as the electricity was gradually diſſipated, the divergency diminiſhed in the ſame proportion. Now, whilſt this diminution of divergency was going on, I looked through a ſmall teleſcope, and by means of a micrometer meaſured the chords of the angles of divergency, ſetting down the time elapſed between each pair of contiguous obſervations; and as the chord of the angle of divarication is in the direct ſimple proportion of the denſity of the electric fluid *, I could by this means know how much electric fluid was loſt by the electrometer in a certain time, and of courſe what portion of the electricity firſt communicated to the electrometer ſtill remained in it. Let us make the chord of the angle of divarication on firſt electrifying the electrometer, or rather when firſt obſerved, equal to 16; or let us conceive that quantity of electricity to be diviſible into 16 equal parts.

I obſerved, that when the chord of the angle became equal to eight, the time elapſed between this and the firſt obſervation was one minute; when the chord became equal to four, the time elapſed between this point and the preceeding obſervation was 3′ 30″; when the chord became equal to two, the time elapſed ſince the preceding obſervation was 17′; and when the chord became equal to one, the time elapſed ſince the

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* This propoſition was firſt aſcertained by F. BECCARIA. See Philoſphical Tranſactions, Vol. LVI.

preceding obſervation was one hour and a quarter; after which the electrometer remained ſenſibly electrified for a long time.

In repeating this experiment, the times elapſed between the correſponding obſervations did not follow ſtrictly the ſame proportion of increaſe; nor did they increaſe regularly in the ſame experiments, which may be attributed in great meaſure to the inaccuracy in obſerving, and to the fluctuating ſtate of the air; but it could be ſafely inferred from all the experiments, that the times required for the diſperſion of the electricity were at leaſt greater than the inverſe duplicate proportion of the denſities of the electricity remaining in the electrometer. And if we imagine, that they continue to diminiſh in the ſame proportion of increaſing time, which is far from being an extravagant ſuppoſition, we ſhall find, by a very eaſy calculation, that about two years after the electrometer would ſtill retain the hundredth part of the electricity communicated to it in the beginning of the experiment; and as we do not know how far a quantity of electricity is diviſible, or to what extent it may be expanded, we may conclude with ſaying that, ſtrictly ſpeaking, the electrometer would remain electrified for many years.

It may be inferred from this, as well as from many other experiments, that the air, or in general any ſubſtance, is a more or leſs perfect conductor of electricity, according as the electricity which is to paſs through it is more or leſs condenſed; ſo that if a given quantity of electric fluid be communicated to a ſmall braſs ball, one may take it away by ſimply touching the ball with a finger; but if the ſame quantity of electric fluid be communicated to a ſurface of about 100 or 1000 ſquare feet, the touching with the finger will hardly take away any part of it.

If it be aſked, what power communicates the electricity, or originally diſturbs the equilibrium of the natural quantity of electric fluid in the various bodies of the univerſe; we may anſwer, that the fluctuating electric ſtate of the air, the paſſage of electrified clouds, the evaporation and condenſation of fluids, and the friction ariſing from divers cauſes, are perpetually acting upon the electric fluid of all bodies, ſo as either to increaſe or diminiſh it, and that to a more conſiderable degree than is generally imagined.

I ſhall laſtly conclude, with briefly propoſing an explanation of the production of electricity by friction, which is dependent upon the above ſtated propoſition, *viz.* that bodies are always electrified in ſome degree; and likewiſe upon the well known principle of the capacity of bodies for holding electric fluid being increaſed by the proximity of other bodies in certain circumſtances.

It ſeems to me, that the cylinder of an electrical machine, like ABC in fig. 4. muſt always retain ſome electricity of the poſitive kind, though not equally denſe in every part of its ſurface; therefore, when the part of it A is ſet contiguous to the rubber *fg*, it muſt induce a negative electricity in the rubber. Now, when by turning the cylinder, another part of it B (which ſuppoſe to have a leſs quantity of poſitive electricity than the preceding part A) comes quickly againſt the rubber; the rubber being already negative, and not being capable of loſing that electricity very quickly, muſt induce a ſtronger poſitive electricity in the part B, which is now oppoſite to it; but this part B cannot become more poſitively electrified, unleſs it receives the electric fluid from ſome other body, and therefore ſome quantity of electric fluid paſſes from the loweſt part of the rubber to the part B of the glaſs, which additional quantity of electric fluid is retained by the part B only whilſt it remains in contact with the rubber; for after that, its capacity being diminiſhed, the electric fluid endeavours to eſcape from it. Thus we may conceive how every other part of the glaſs acquires the electric fluid, &c. and what is ſaid of the cylinder of an electrical machine may, with proper changes, be applied to any other electric and its rubber.

It appears, therefore, that according to this theory, a part of the rubber, *viz.* that which the ſurface of the glaſs cylinder enters in turning round, muſt ſerve to furniſh the electric fluid to the glaſs, and the upper part muſt be poſſeſſed of a negative electricity capable of inducing a poſitive electricity in the glaſs contiguous to it. In fact, this ſeems to be confirmed by the general practice and experience; for that rubber anſwers beſt for an uſual electrical machine, which can eaſily conduct the electric fluid with its under part, and the upper part of which is more ready to acquire, and to retain, the negative electricity; hence the rubbers are generally furniſhed with amalgam below, and with a piece of ſilk above; hence alſo, if the cylinder of the machine be turned the contrary way, it will produce little or no electricity.

It often happens, that the part which conducts the fluid, and that which acquires the electricity contrary to that of the electric, are not ſo diſpoſed in a rubber as the one above deſcribed; but it remains always true, that the rubber muſt be poſſeſſed of thoſe two properties, *viz.* to conduct the electric fluid very readily in one or more parts, and to acquire, as well as retain, on other parts, an electricity contrary to that acquired by the electric that is to be rubbed with it.

Little St. Martin's-Lane,
Nov. 7, 1787.

T. CAVALLO.

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Philos. Trans. Vol. LXXVIII. *Tab.* I. *p.* 22.

Fig. 4.

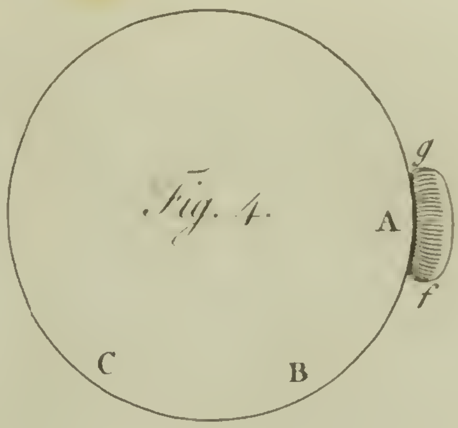


Fig. 1.

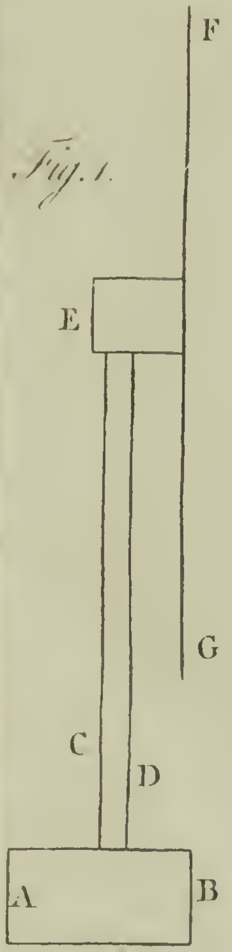


Fig. 2.

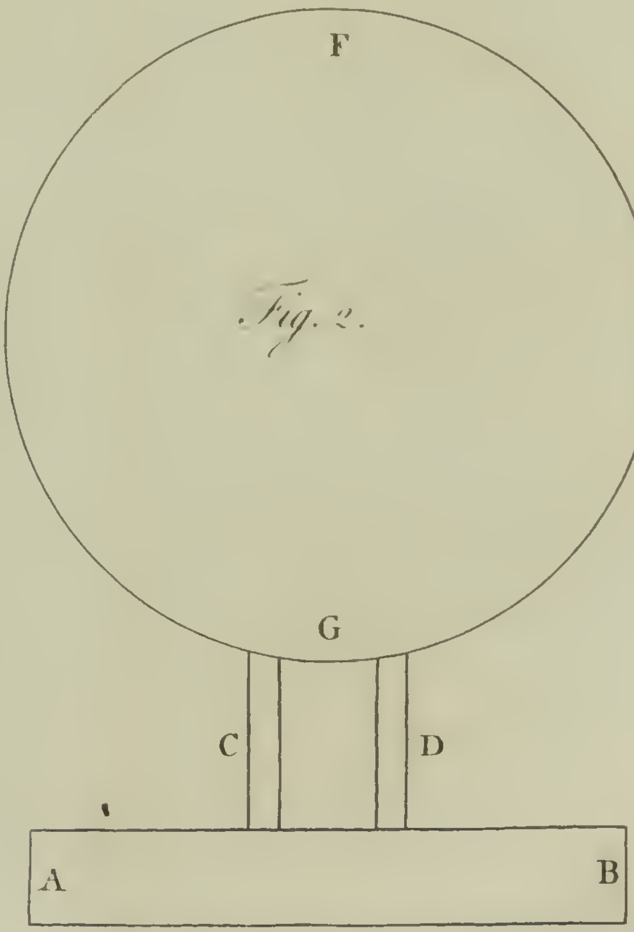
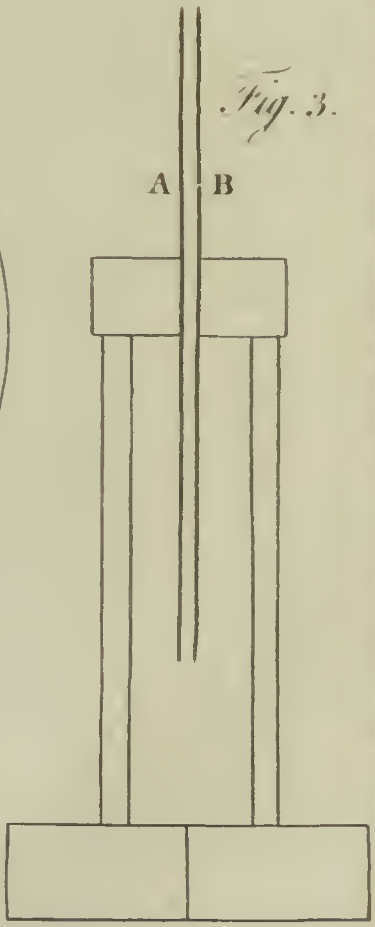


Fig. 3.



T. Cavallo del.

BRITISH MUSEUM
NATURAL HISTORY.

II. *The Croonian Lecture on Muſcular Motion.*

By George Fordyce, *M. D. F. R. S.*

Read November 22, 1787.

THE ſubject of muſcular motion has been ſo often conſidered, and in ſo many lights, that it is hardly poſſible to avoid many obſervations which are trite, and even puerile; which, when they occur, I beg the learned Society to forgive. They will alſo find ſo little new, or even nothing, that I ſhould be inexcuſable in taking up their time, if the ſubject was not annually to be diſcuſſed before them. In conſidering muſcular motion, I muſt begin with ſome obſervations on motion in general, and with that well known, and ſelf-evident axiom, that one particle of matter, conſidered by itſelf, will remain at reſt if it be at reſt, and will continue in motion if it be in motion, and in the ſame direction. This has been called the *vis inſita*, or *vis inertiæ*, of matter. It may be ſaid in other words, that a ſingle particle of matter being at reſt, would therefore always continue at reſt, if it were not for ſome external impulſe made on it. This impulſe may be from ſome other particle of matter in motion impinging upon it, and communicating part of its motion to it, while it communicates an equal quantity of its reſt to the matter ſo impinging upon it, ſo that the quantity of motion and reſt ſhall be the ſame after the impinging, in both bodies, as they were before: or, in other words, a ſimple particle of matter in motion would always continue in motion, in the ſame direction, if it did not meet with another, on which it impinged; and after the impinging, there would be the ſame quantity of motion and reſt in both bodies taken together, as was before. If we conſider equal motion, in direct contrary direction, as reſt; motion, or reſt, produced in a body by the above means, I ſhall call communicated.

If two ſimple particles of matter, of any ſpecies, not farther diſtant from one another than the ſun is from the earth, were both at perfect reſt, theſe two particles would inſtantly begin to move toward one another, if no other particle of matter whatever exiſted.

I do not mean to tire my learned Audience with demonſtration of propoſitions ſo well known.

There would, therefore, be an impulſe, producing motion between theſe bodies, without any contact.

Motions produced in this way, I call original motions.

The firſt conſideration with regard to any particular motion is, therefore, whether it be an original or communicated motion. If it be an original motion, it will follow the laws of that particular ſpecies of original motion; if it be a communicated motion, it will follow the laws of communicated motion.

Many obſervations ſhew, that muſcular motion is not a communicated motion, and therefore an original one.

In any ſyſtem of bodies, or particles of matter, affecting one another only by the motions already exiſting in them being communicated to one another, they may diminiſh their motion, or bring one another to reſt; but they never can increaſe the motion exiſting in the whole. It happens frequently, that the motions in the animal body are increaſed, without any alteration of external applications to it; the caſes are ſo numerous, that it is hardly worth bringing an example: we might mention the increaſe at times of the circulation, and all the motions of the fluids without the leaſt new motion in the ſurrounding bodies, or interference, or even knowledge of the mind. This motion muſt therefore be original, and not communicated.

In communicated motion, if one body be at reſt, and a motion be communicated to it by another, the power of the whole motion ſhall not be greater than that in the communicating body at the time of the communication. If I take out the heart of an animal, cut off the auricles, it will in many caſes continue to contract and dilate for ſome time. If it be left to come to reſt, and if ſoon after a needle be introduced into the ventricle, placed tranſverſely, and if the interior ſurface of the ventricle be pricked gently by the needle, the ventricle will contract with ſuch power as to force the needle deep into it: in this caſe, the force of the contraction of the ventricle is much greater than the power with which it was pricked by the needle; this contraction was therefore not communicated to it by the moving needle, but was generated, and therefore an original motion.

In all communicated motion, by which two bodies at a diſtance are brought near to one another, there muſt ſubſiſt ſome other matter, by which they may be drawn, or forced, nearer to one another; but in original motion, it is not at all neceſſary that any other matter ſhould exiſt at all. Two particles, placed at as great a diſtance from one another as the ſun is from the earth, as has been already obſerved, although at perfect reſt, would begin to move nearer one another, by the attraction of gravitation, if all other matter whatever were annihilated.

Moſt authors who have treated on muſcular motion have ſuppoſed, that it was a communicated motion; and that it was produced by ſomething paſſing by the nerves, from the brain to the moving part. Three doctrines have been ſet forth; one, that there is a fluid paſſing along the nerves; a ſecond, that there is a vibration; and a third, that the nerves are ſurrounded with ſomething like electric matter, in which motion runs from the brain to the moving parts. Thoſe who have conſidered this ſubject muſt be tired of the arguments which have been brought to refute each of theſe; for no argument from fact has been employed to prove any one of them: I ſhall therefore leave them as mere chimeras of the brain. I have taken notice, that it is not requiſite for any motion to paſs between two bodies, exciting in each other an original motion, through or by any other matter: I have alſo ſhewn, that muſcular motion is an original motion: it follows, that it is not neceſſary for any motion, or communication, to paſs through any other matter, in order to bring the muſcular fibres into action.

One caſe of muſcular motion is, when a ſtimulus is applied to ſome part of the body, and a muſcle at a diſtance immediately contracts. It has been ſuppoſed in this caſe, that ſome influence was communicated to the nerve of the part where the ſtimulus was applied, and through it to the brain, and from the brain through the nerves of the contracting muſcle. Granting, for a little, that ſome motion may paſs along the nerves, and therefore that the end of the nerve, where the application was made, may be the part in which the original motion began, the ſtimulus frequently does not touch the end of the nerve; for if vapour of volatile alkali be applied to the noſtrils, an univerſal glow of heat, and increaſed circulation, will conſtantly take place; but the vapour of the volatile alkali could not touch the nerves of the noſtrils, the membrane being conſtantly covered with mucus, which the vapour could not penetrate without diſſolving it, which it had not time to do, and, if it had, would have united with it, ſo as to form a ſoap, void of any ſtimulating power.

If, therefore, the original motion began in the end of the nerves of the noſtril, it muſt be excited by a ſubſtance at a diſtance from the end of that nerve, without any communication of motion between the ſtimulus, that is, application producing the motion, and the end of the nerve in which it is excited; therefore, on any ſuppoſition, a ſtimulus is capable of exciting a motion, in a part at a diſtance, without any communication of motion; and it is therefore not neceſſary, that the nerves ſhould be at all employed in the motions of the body excited by a ſtimulus, as it can act at a diſtance without their intervening. Further, that the nerves are not employed in the motions excited by ſtimuli, is evident from this experiment: take the heart out of a living animal, cut all the nerves off as cloſe as poſſible, lay it in nearly the heat of the body of the animal, it will continue to contract for ſome time. As ſoon as it has ceaſed contracting, prick a fibre in one of the ventricles; both ventricles, and all their fibres, will contract inſtantly, although there be now no communication by the nerves, between many of the contracting fibres, and the fibre ſtimulated. It might be ſuſpected, that the motion of the fibre ſtimulated might affect the others: in this caſe the contractions would be progreſſive; but, on the contrary, the whole contract at once.

I cannot help bringing another inſtance, where ſtimuli produce action in parts at a diſtance, without any communication of motion by the nerves. When infuſion of cantharides is applied to the ſkin, as we ſay vulgarly, it is not applied immediately to the ſkin; but in the firſt inſtance to the mucous and ſebaceous matter, which every where covers the ſcarf ſkin; under this lies the ſcarf ſkin, which the infuſion can hardly be conceived to come at; if it did, the ſcarf ſkin we know is perfectly impenetrable to ſuch a fluid; it can therefore never touch the ſkin, in which it excites inflammation, and on which it therefore acts at a diſtance, and excites motion, which no one can ſuſpect to come through the nerves: nor is there any motion through the mucus and ſcarf ſkin, of any other kind than would ariſe if an infuſion of any other inſect had been applied, which had no power of exciting inflammation.

From what has been ſaid we may conclude, that when a ſtimulus has been applied ſo as to excite motion in a diſtant part, no motion whatever takes place in the nerves, or is communicated by them from the part to which the ſtimulus is applied to the moving part.

I need not draw your attention to another propoſition, *viz.* that when a ſtimulus is applied to a diſtant part, ſo as to produce motion, it often happens, that the ſtimulating matter is not carried by the blood-veſſels, or otherwiſe, to the moving part. This propoſition has often been demonſtrated, and is well known.

All the original power exerted by any of the moving parts conſiſts in a power of particles coming nearer one another; for every muſcle or fibre becomes ſhorter when it acts; or, in other words, contracts; and every other moving part in like manner contracts when in action. It is true, that there are many contrivances to make the contraction have great effect in producing motion, force being never ſpared for conveniency, as my friend Mr. HUNTER has, I believe, already ſet forth to my learned audience: but, nevertheleſs, it is clear from every experiment made on the ſubject, that all motion ariſes from particles coming near one another in ſome direction. It would be ſuperfluous for me to point out theſe experiments; I ſhall mention one only, and an obvious one. Lay bare a muſcle, and prick any of its fibres, it immediately becomes ſhorter.

The original power of coming nearer to one another of two or more particles of matter, has been called attraction. There have been ſeveral original powers of coming nearer one another of particles of matter, which have been conſidered as different attractions, ſuch as the attraction of gravitation, of magnetiſm, of electricity, &c.; all of which this Society are too much maſters off, for me to enter into any diſquiſition with regard to them.

The attraction which is my preſent object, I call the attraction of life. This attraction is either of two ſpecies, or is exerted variouſly; for all the moving parts have their particles nearer one another in the living than in the dead body. The proof of this is as neceſſary as it is obvious. I muſt, therefore, take up a ſmall portion of your time in pointing it out. Take the body of any animal, when the life is entirely gone from it, and the effects of it are entirely loſt, but before any putrefaction, or any change in its chemical qualities, has taken place; and lay bare, and diſſect out, any muſcle, eſpecially one which has long fibres, and no middle tendon, ſuch as the *ſartorius*, for example, and afterwards lay it in its place, leaving it of the length it naturally takes; it will reach farther than from its origin to its inſertion; but lay bare, and diſſect out, the ſame muſcle in the living body, and it will always be ſhorter than from its origin to its inſertion. If it ſhould be ſaid, that the diſſection ſtimulated the muſcle, and brought it into action, let it not be diſſected out, but its tendon cut through, as the

Tendo Achillis, for inſtance, the ſame thing will happen. And we now are all convinced, from various experiments, that a tendon in a ſound ſtate is not capable of being ſtimulated by being wounded, cut through, or broken.

I apprehend, then, that we may conclude, that all the moving parts are conſtantly contracted, that is, their particles are nearer one another when the body is alive, than when dead, and totally left to their elaſticity. This ſpecies of action I call the tone. The ſecond ſpecies, or variety, which occurs in the attraction of life, is when a moving part, for a ſhort time, has its particles brought nearer one another than they are from their tone, and which very rarely continues for many ſeconds of time without intermediate relaxation. I call it their action; when it does continue for a longer time, it is called ſpaſm; which, however, is ſo vague a term, that I could wiſh totally to reject it, at leaſt to confine it only to this ſenſe, *viz.* a greater contraction, or coming nearer one another of the particles, of a moving part, than that which would happen from their tone, remaining without any intermediate relaxation.

I for the preſent do not mean to ſay any thing farther with regard to the tone, or ſpaſm of parts; but only to call to your conſideration the action, as excited by applications to ſome part of the body at a diſtance from the moving part. I have already rejected all communication by the application, or ſtimulus, being carried by the blood-veſſels, or any other way whatever, to the part. I have alſo rejected any motion, or communication of any kind whatever by the brain and nerves to the part. I conceive, that when any ſtimulus or application whatever is made in any part, ſo as to produce any action in a diſtant part, that that medicine or application, without having any operation whatever on the intermediate parts, gives a power to the particles of the moving part of greater attraction. I ſhall illuſtrate this idea by ſuppoſing, that there is a machine moving by various powers, either original or communicated; and that in this machine there are two magnets, which by their attractive power have come to a given diſtance from one another, but have been prevented from coming nearer by ſome power endeavouring to draw them back. A much ſtronger magnet applied to a part of the machine, in a certain manner, ſo as not to touch either of the two already there, nor to affect any other part, may increaſe their power of attraction, ſo as to make them overcome the reſiſtance, and come nearer one another *.

In the ſame manner, I apprehend, that an application made to the ſkin of the abdomen may, and often does, occaſion the action of the inteſtines to take place, without any effect whatever on the intermediate parts; but that it ſimply excites the attraction of the particles of the moving parts of the inteſtines; certainly a part of the matter through which the influence is to paſs, *viz*, the mucus and ſcarf ſkin, is actually inanimate matter.

In certain caſes of original motion, there is attraction, or the coming nearer of particles only. In others, there is not only attraction, but oppoſite repulſion; the caſes of which are unneceſſary to enumerate to this Society. In the attraction of life there is no oppoſite repulſion; all the motions of the body are produced entirely by the force of particles coming nearer one another. When this force diminiſhes, or the action goes off, and leaves the part entirely to its tone, the particles of the moving part are by no means repulſed from one another, but are left to be drawn aſunder by their elaſticity, weight, or the weight of the ſurrounding parts, or any other accidental power: but, nevertheleſs, there are applications which may be made to diſtant parts of the body, which may, and do, take off the attraction which occaſions the action of the moving part; and all thoſe reaſonings which I have already applied to applications which excite action, and which are called ſtimuli, are equally applicable to thoſe applications which make action ceaſe, and which we call ſedatives.

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* I do not mean to inſinuate, in the ſmalleſt degree, that the powers of the body at all depend on, or have any thing to do with, magnetiſm.

The great ground on which I have attempted to make theſe obſervations, is the foundation of certain maxims in the practice of medecine, which I ſhall now proceed to ſketch out to the Society, whoſe inſtitution, to me, has always ſeemed to include the philoſophy, but not the actual practice, of medicine.

Medicine is a ſcience of long cultivation in that channel in which all the ſciences have flowed, and had early attained great perfection, I believe, from the teſtimony of various writers of antiquity, and other circumſtances, which I paſs over as well known to many of this learned body, and too foreign a digreſſion from our preſent ſubject: for although CELSUS obſerves well, that there could be no phyſicians among the Greeks at the time of the Trojan war, inaſmuch as HOMER never mentions one medicine, but only application to the Gods for the cure of fevers, and other internal diſeaſes; yet the Egyptians, from whom the Greeks received a great part of their knowledge in all ſcience, as well as in medicine, had certainly not only regular phyſicians for internal diſeaſes, but likewiſe ſtone-cutters, oculiſts, auriſts, &c. long before the Trojan war; and HIPPOCRATES, by his own teſtimony, took much of his knowledge from what he calls the ancients. In the progreſs, therefore, of the ſcience of medicine, it came into my mind to enquire how far, and on what ground, the modern increaſe of ſcience in anatomy, chemiſtry, mathematics, &c. had forwarded the knowledge of medicine. In the firſt place, it is well known to my learned Audience, that medicine was in the hands of Greek phyſicians from the time of HIPPOCRATES, or rather from the deſtruction of the Egyptian monarchy by CAMBYSES, down to the time of the Cruſades; in all this time there was hardly a diſſection of the human body, from an opinion about *manes*; but when it came into Europe again, where this opinion remained indeed, but in a much leſs degree, anatomy began again to flouriſh; and by other means all the other ſciences ſhone forth with a greater luſtre than they had ever done in any period handed down to us by hiſtory of any nation. It was obvious therefore to conceive, that the knowledge of the ſtructure of the body, and the inveſtigation of the powers of matter, made in a more accurate manner, and on a more extenſive ſcale, would elucidate the doctrine of the human body, and its diſeaſes, and their treatment, in a new and more perfect manner: to this opinion I mean now to apply the reaſoning I have before laid down.

In the ſtructure and phyſiology of the body, two great diſcoveries have been made by the moderns; the circulation of the blood; and the lymphatics, and abſorption of the lymph. Theſe at once overthrow the ideas of the ancients with regard to moſt of the functions of the interior parts of the body; which was now conceived by many to be an hydraulic machine, and ſubject to all thoſe diſorders which were incidental to ſuch a machine; and particularly, from various fluids flowing with great rapidity, through tubes, many of them of infinite fineneſs, that ſtoppages muſt often take place, which were to be removed by diſſolving out the obſtructing matter; that the blood was mixed ſo perfectly through all the body, and ſo conſtantly, that the ſame blood muſt be taken away, whatever blood-veſſel was opened; and when we contemplate the numerous openings and communications of the veſſels with one another, that blood flowing out of any one will empty them all equally, and that, therefore, it can be of no conſequence from what part of the body blood is evacuated. In a pleuriſy, for inſtance, where can be the difference, whether blood be taken from the right or left arm, or from the veſſels of the ſkin of the breaſt? But there is a difference, and a great one too; ſince taking a much leſs quantity of blood from the ſkin of the breaſt, is actually known, in certain caſes, from experience, to cure a pleuriſy, than would have had that effect if taken from the veſſels in the arm, and will even carry off the diſeaſe, when it could not be carried off at all by evacuation from the arm: yet it is undoubtedly the very ſame blood in all its qualities; and in both caſes the veſſels of the pleura are equally emptied. The act of flowing out of the blood from the veſſels of the ſkin of the breaſt then has an immediate action on the action of the moving parts in the pleura, and carries off the inflammation independent of the circulation, or any of its laws; and ſo far has the knowledge of the circulation been of any advantage in this caſe, that it had nearly thrown out topical bleeding in inflammation, which is one of our moſt powerful remedies in the diſeaſe. In like manner, when the moving fibres of the ſtomach do not contract, ſo as to expel any vapour that may get into it, a ſpice applied to the ſkin over the ſtomach will, in many caſes, occaſion theſe fibres to contract. Now it is well known from anatomy, that there is no communication between the ſkin of the abdomen and the ſtomach; and if the ſpice were to act by touching theſe fibres, it would be the ſame, whether it was applied to the ſkin over the ſtomach, or to the ſkin of the arm; for in both caſes it muſt be abſorbed by the lymphatics, and carried to the left ſide of the heart, and there and in the lungs be blended univerſally with the whole blood, and carried by the arteries to the moving fibres of the ſtomach. Nay more, that it would be equal, whether it was applied to the inner ſurface of the ſtomach itſelf, or to the external ſkin, or any other membrane, of any other cavity: for the ſtomach is covered with mucus, and lined with a membrane which is perfectly impervious, and totally prevents any thing contained in the ſtomach from being any way applied to the moving fibres; it muſt in this caſe, therefore, be likewiſe taken up by the lymphatics, or lacteals, and carried to the heart before it could touch the moving fibres of the ſtomach. The maxim then, ariſing from our knowledge of the lymphatics, would be, that it was of no conſequence where we applied a ſpice in caſes of flatulency; which is not true. By ſimilar reaſons it might be eaſily ſhewn, were it not for tiring my learned Audience, that all the knowledge of the properties of the fluids, which has been acquired by modern and accurate experiments, hardly contributes any thing to the knowledge of applying medicines for the cure of diſeaſes; and that the ſtudy of the laws of the attraction of life, or what has been called muſcular motion, is of conſiderable importance.

One more obſervation I have only to make, *viz.* that all original motions are by their nature perfectly unintelligible as to their cauſe; who can tell the cauſe of gravity, chemical attraction, &c.? and ſo undoubtedly the attraction of life, in its cauſe can never be inveſtigated, being, like all other attraction, a power which two or more particles of matter have of coming nearer one another. But although the ſtudy of cauſes of original powers be totally abſurd and futile, yet the laws of their action are capable of inveſtigation by experiment, and applicable to the evolving much uſeful knowledge. Need I remind this Society, that the inveſtigation of the laws of gravitation, by Sir ISAAC NEWTON, has rendered it immortal? The inveſtigation of the laws of the attraction of life has alſo been greatly forwarded by one of its preſent members; but it is an inveſtigation which will probably require ages to render perfect,

III. *An Account of a Maſs of native Iron, found in South-America.* *By Don* Michael Rubin de Celis. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 22, 1787.

Memoria que remite a la R. Sociedad de Londres D. MIGUEL RUBIN DE CELIS, Cavellero del Orden de Santiago, Teniente de Fragata de la R. Armada de S. M. C. Academico correſpondiente de la R. Academia de Marina de Francia, y de la R. de la Hiſtoria de Eſpaña.

HAZE mas de treinta años que en las provincias del gran Chaco Gualamba, de adonde varias Naciones barbaras arrojaron a los Eſpañoles, y deſde cuio tiempo eſtà quaſi deſierta la parte meridional del Rio Bermejo, y occidental del gran Paranà, los pocos Indios que ſe hallan en la juriſdicion de Santiago del Eſtero, teniendo por unico exercicio el ſacar Miel y Cera de los montes, de la que hay una cantidad prodigioſa en los arboles, deſcubrieron en el medio de un campo un trozo de metal que llamaron fierro puro, y que ſobreſalia de la tierra por una banda coſa de un pie, y ſe veìa quaſi toda ſu cara ſuperior: anuncioſe a los Virreyes del Peru, como un deſcubrimiento raro, y con razon al parezer, pues donde no hay montañas, ni la mas pequeña piedra en cien leguas al rededor, una coſa ſemejante merezia el nombre de maravilloſa, ſin embargo de conſtar haber en Europa minas de fierro puro. Hubo particulares que con rieſgo eminente de ſus vidas, ya por la eſcaſez de aguas, que no hay ſino la que en tal qual cavidad natural ſe recoje de las llubias, ya por la eſcazez de viveres, ya por el rieſgo de los Indios barbaros, ya por los varios animales ferozes, como Tigres, Leopardos y Antas, ya por los muchos reptiles ponzoñoſos, ya enfin por las infinitas eſpeſuras, ſe arrojaron, quiza ſeducidos de la codicia, à hazer la trabeſia para extraher algo de la materia: remitieron de ella à Lima y Madrid, donde no ſe adelantò otra coſa que ponerla el nombre de fierro mui dulze y mui puro. Como que el fierro, por razon politica, no ſe trabaja en aquel continente, aunque de el hay muchas minas, y como que ſe aſeguraba que la veta del fierro corria muchas leguas, ſiendo aſi que lo viſto ſolo era un creſton ſobreſaliente à la tierra, y que deſpues de eſcavado tenia tres varas de N. à S. dos y media de E. al O. y una terzia de alto, me deſtinò el Virrey del Rio de la Plata à que examinaſe prolijamente la dicha maſa, y en el caſo de ſer mina, y util, proyectaſe ſobre Poblacion. Sali con competente eſcolta, a principios de febrero de ochenta y tres, del Rio Salado, antigua reduccion de los Indios Vilelas, ſiguiendo el Rumbo de E. ¼ N.E. bien que debi ſeguir el del E. ¼ S.E. todo corregido.

El aſpecto del pais que media entre el Rio y la Mina, diſtante ſetenta leguas de la Reduccion, es mui curioſa; una llanura immenſa, alternada de montes eſpeciſſimos y campos fertiles, forma la mas agradable perſpectiba.

Obſervè la latitud de la mina 27 gr. y 28 min.

No hay Poblacion alguna en toda la traveſia, por eſcaſez de aguas manantiales: la que gaſtan los meleros, que en pequeñas caravanas reſiden la maior parte del año meleando en los montes, es la llovediza como ſe ha expreſado. Eſtos y algunas tribus de Indios errantes y barbaros, cuia vida es ſemejante a la de los Tartaros, que en cierta eſtacion del año vienen de las marjenes del Rio Vermejo à cabar o ſacar una raiz ſilveſtre llamada Koruu: que ſolo la maſcan continuamente, ſiendo para ellos de primera neceſſidad, aſi para preſerbarſe de los efectos de una atmoſphera infeſtada, como de las mordeduras continuas de los reptiles ponzoñoſos: ſon las unicas gentes que ſe hallan en eſtos dilatadiſſimos y amenos Campos.

El quinze lleguè a los nombrados de Otumpa, donde ſe hallava la maſa quaſi enterrada en pura greda y cenizas.

La apariencia exterior era de fierro perfectamente compacto; no aſſi la interior, como puede verſe en las cavidades que manifieſtan los cortes de los pedazitos, lo que induze à creer haber eſtado liquida en algun tiempo: fortifica eſta idea una porcion de trazas exteriores en la parte ſuperior; ſe hallan las de pies y manos de Hombres de crecida eſtatura, las de pies de varios paxaros grandes communes en aquellos campos: y aunque perfectas las tales trazas no dejo de perſuadirme a una de dos coſas, obien que ſea un capricho ò accidente natural, obien que en el ſuelo, antes de caer alli aquella maſa, exiſtieren las talles huellas, y que en la caida de la maſa liquida hubieſen reſultado en la parte ſuperior. A nada ſe aſemeja tanto como a un amaſijo de pan que, deſpues de haberſe en el eſtampado pies manos y metido un dedo, ſe hubieſe ferrificado.

Empezé à cortar con los cinceles, y para ſacar veinte y cinco, ò treinta libras, fue neceſſario romperlos todos en numero de ſetenta.

Hize cavar al rededor del trozo, quien por la parte inferior tenia una capa del grueſo de quatro ò ſeis pulgadas de eſcoria, procedida de las humedades de la tierra; por la ſuperior eſtava limpio.

Deſprendido de la tierra, le hize dar una media buelta con palancas, y cabè en la cama a baſtante profundidad. No contento conſtruì dos hornillos, que volè, y con cuidado examinè la tierra mas profunda, y la hallè perfectamente ſemejante a la ſuperior, a la de todos aquellos campos, y a la de dos pozos que hize abrir a diſtancia de ſeſenta ò cien paſos del trozo y direccion del E. O.

El no encontrar raiz ni ſerie de generacion, avivò mas la admiracion mia, y empeze a diſcurrir de eſte modo.

O eſta maſa ſe crio aqui, o fue traìda ò arrojada? Criarſe aqui no es poſible, ſegun el conocido proceder de la Naturaleza: ſer trahida de donde, para que, ò como? de donde, no haviendo en centenares de leguas minas de fierro, ni memoria de que ſe hubieſen trabajado en el Reyno? para que? ſiendo de una materia ſin eſtimacion, no pudiendoſe uſar, y ſiendo aquel un terreno el mas eſcuſado o inavitable de todo el Chaco, por la eſcaſez de aguas; como? no teniendo noticia de que los Indios hayan conocido jamas las maquinas de acarreo; con que habrà ſido arrojada por algun volcan.

Las razones que induzen a creer eſto ſon varias. Regularmente los volcanes dejan deſpues de volados algunos pozos de agua, unas vezes termales, y otras frias: al E. del trozo, y diſtancia como de dos leguas, ſe halla vna aguadita baſtante ſalobre vnica y manancial por todo aquel pais. En todo lo que caminè del Chaco no obſerbè a la viſta diferencia ninguna de nivel en el terreno: ſolo en la imediacion à eſta aguadita hay vna rampa que corre N. S. y que por particular ſeme hizo notable antes de ver el trozo. Graduando poder tener de diferencia de nivel con el reſto del pais de quatro ò ſeis pies: las imediaciones a la papa ò trozo, y a la aguadita, eſtan cubiertas de una eſpecie de tierra menuda como la ceniza, y quaſi de ſu color: el Paſto de eſtas imediaciones, llamado Ahuvi, es ſumamente pequeño, delgado, y aborrecido de los animales, aſi como mui alto y mui grato a ellos el que eſtà algo ſeparado del trozo y de la aguadita: à pequeña profundidad en eſta ſe encuentran unas piedras quarzos de un encarnado hermoſo, las que han uſado los meleros ſolo para encender fuego: hay noticia de haber aquellos trahido tres pequeñas por particulares en ſu color, y unas pintas doradas; una de ellas del peſo de una onza vino a poder del Governador de Armas de Santiago del Eſtero, quien me dijo haberla echo moler, y me moſtrò mas de un adarme de oro que de ella ſe havia extraido. Es indudable que entre aquellos immenſos boſques exiſte un Arbol con ſus ramas de puro fierro; ſon muchos los Indios que le han viſto, y los de la reduccion llamada de Avipones ſaben ſu verdadero lugar: un Europeo diſtinguido vezino de la Cuidad de Salta lo ha tocado: la caida o inclinacion del trozo es del E. al O. haviendo dejado vna eſpecie de corte al E. cuia direction es N. S. de eſte corte es de donde ſe pudieron extraer con cinceles los pedazitos de metal: luego con eſtos antecedentes no es violento formar eſta Ypoteſi.

Voloſe el volcan en la aguadita ſalobre, lebantò vna porcion de tierras que por ſu poca gravedad ſe quedaron en la imediacion; aunque al principio amontonadas, las continuas aguas del Chaco, que ſe halla inundado la terzera parte del año, las ha ido igualando con el otro terreno: la direccion de la materia grande arrojada fue del E. al O. y como peſada alcanzò a la diſtancia en que ſe encuentra: alguna otra porcion de materia mas pequeña, y acaſo mas fluida, ſe ſeparò a otro rumbo, haciendo canales, al modo que quando ſe arroja un baſo de agua; eſta pequeña materia ya fria, y lamida con las aguas la tierra que al principio la ſoſtenia, devio de formar lo que hoy ſe llama arbol de fierro: las materias ſalitroſas y antimoniales, que accompañan de ordinario à todo mineral, devieron igualmente eſparcirſe y infertelizar los contornos.

En el Reyno de Santa Fee de Bogotà ſe encuentra la Platiña en polbo meſclada con el oro: pocos ignoran la mucha afinidad que tienen eſtos dos metales, con que no ſeria extraño, ſegun el conjunto de todas eſtas razones, que el fuego del volcan hubieſe derretido la Platina, y arrojadola como materia ſuperior a la de oro, que era inferior. Eſte principio de los volcanes es el mas natural para comprobar el porque ſe hallan tambien ſueltas las afamadas papas de Plata de Guantajaia, ſobre cuia formacion ſe han dicho exquiſitas extravagancias y ridiculezes.

Cubicado ſobre poco mas o menos, reputando la gravedad expecifica del metal algo maior como lo es que la del fierro, vendrà a peſar mas de treſcientos quintales.

El Cav. MIG. RUBIN DE CELIS.

R. Iſla de Leon,
2 de Junio de 1786.

Some ſpecimens of the Iron accompanied this Paper, and were laid before the Society; who afterwards preſented them to the Britiſh Muſeum. C. B.

IV. Frigorific Experiments on the mechanical expansion of Air, *explaining the Cauſe of the great Degree of Cold on the Summits of high Mountains, the ſudden Condenſation of aerial Vapour, and of the perpetual Mutability of atmoſpheric Heat.* *By* Eraſmus Darwin, *M. D. F. R. S.*; *communicated by the Right Honourable* Charles Greville, *F. R. S.*

Read December 13, 1787.

HAVING often revolved in my mind the great degree of cold producible by the well known experiments on evaporation; in which, by the expanſion of a few drops of ether into vapour, a thermometer may be ſunk much below the freezing point; and recollecting at the ſame time the great quantity of heat which is neceſſary to evaporate or convert into ſteam a few ounces of boiling water; I was led to ſuſpect, that elaſtic fluids, when they were mechanically expanded, would attract or abſorb heat from the bodies in their vicinity; and that, when they were mechanically condenſed, the fluid matter of heat would be preſſed out of them, and diffuſed among the adjacent bodies.

As this principle might poſſibly be extended to elaſtic ſolid bodies, as well as to fluid ones, and explain the cauſe of the heat occaſioned by percuſſion or friction, and by ſome chemical combinations, as well as the perpetual mutability of it in the atmoſphere, I have, at different times, endeavoured to ſubject it to experiment.

1. When Dr. HUTTON of Edinburgh, and Mr. EDGEWORTH of Edgeworthtown in Ireland, were with me about twelve or fourteen years ago, the following experiment, which had been propoſed by one of the company, was carefully made. The blaſt from an air-gun was repeatedly thrown on the bulb of a thermometer, and it uniformly ſunk it about two degrees. The thermometer was firmly fixed againſt a wall, and the air-gun, after being charged, was left for an hour in its vicinity, that it might previouſly loſe the heat acquired in the act of charging; the air was then diſcharged in a continued ſtream on the bulb of the thermometer, and the event ſhewed, that the air at the time of its expanſion attracted or abſorbed heat from the mercury of the thermometer.

In March 1785, by the aſſiſtance of Mr. FOX and Mr. STRUTT, of Derby, a thermometer was fixed in a wooden tube, and ſo applied to the receiver of an air-gun, that, on diſcharging the air by means of a ſcrew preſſing on the valve of the receiver, a continued ſtream of air, at the very time of its expanſion, paſſed over the bulb of the thermometer. This experiment was four times repeated in the preſence of many obſervers, and uniformly ſunk the thermometer from five to ſeven degrees. During the time of condenſing the air into the receiver, there was a great difference in the heat, as perceived by the hand, at the two ends of the condenſing ſyringe; that next the air-globe was almoſt painful to the touch; and the globe itſelf became hotter than could have been expected from its contact with the ſyringe. Add to this, that in exploding an air-gun, the ſtream of air always becomes viſible, which is owing to the cold then produced precipitating the vapour it contained; and if this ſtream of air had previouſly been more condenſed, or in greater quantity, ſo as not inſtantly to acquire heat from the common atmoſphere in its vicinity, it would probably have fallen in ſnow, as in the fountain of Hiero, mentioned below.

2. About twelve or fourteen years ago, by the aſſiſtance of Mr. WALTIRE, a celebrated itinerant teacher of philoſophy, a thermometer was placed in the receiver of an air-pump, and ſome time being allowed, that it might accurately adapt itſelf to the heat of the receiver, the air was haſtily exhauſted; during which the mercury of the thermometer ſunk two or three degrees, and after ſome minutes regained its previous height. In November 1787, by the aſſiſtance of my very ingenious friend Mr. FORESTER FRENCH, the above experiment was repeated; but with this difference, that the thermometer was open at the top; ſo that the diminution of external preſſure could not affect the dimenſions of the bulb; and the reſult was the ſame, the mercury in the thermometer ſunk two or three degrees, and gradually roſe again. Does not this ſhew, that the air in the receiver, being expanded during the exhauſtion, attracted or abſorbed heat from the mercury in the thermometer?

Both during the exhauſtion, and during the re-admiſſion of the air into the receiver, a ſteam was regularly obſerved to be condenſed on the ſides of the glaſs, which in both caſes was in a few minutes re-abſorbed. This ſteam muſt have been precipitated by its being deprived of its heat by the expanded air: if it could have happened from any other cauſe, the vapour could not, in both ſituations, *viz.* of exhauſtion, and of re-admiſſion, have been taken up again.

3. In-

3. In December 1784, with the aſſiſtance of Mr. Fox, the following experiment was carefully made. A hole, about the ſize of a crow-quill, was bored into a large air-veſſel, placed at the commencement of the principal pipe in the waterworks which ſupply the town of Derby. The water from four pumps, which are worked by a water-wheel, is firſt thrown into the lower part of this air-veſſel, and from thence riſes to the top of St. Michael's Church into a reſervoir, which may be about thirty-five or forty feet above the level of the air-veſſel.

Two thermometers were previouſly ſuſpended on the leaden air-veſſel, that they might become of the ſame temperature with it; and, as ſoon as the hole was opened, had their bulbs reciprocally applied ſo as to receive the ſtream of air; and the mercury in both of them ſunk two diviſions, or four degrees. This ſinking of the mercury in the thermometers could not be aſcribed to any evaporation of moiſture from their ſurfaces, becauſe it was ſeen, both in exhauſting and re-admitting the air into the exhauſted receiver, that the vapour which it previouſly contained was depoſited during its expanſion.

4. There is a very curious phænomenon obſerved in the fountain of Hiero, conſtructed on a very large ſcale in the Chemnicenſian mines in Hungary, which is very ſimilar to the experiments above related. In this machine the air, in a large veſſel, is compreſſed by a column of water 260 feet high: a ſtop-cock is then opened, and as the air iſſues out with great vehemence, and, in conſequence of its previous condenſation, becomes immediately much expanded, the moiſture it contained is not only precipitated, as in the exhauſted receiver above-mentioned, but falls down in a ſhower of ſnow, with icicles, adhering to the noſel of the cock. This remarkable circum-

ſtance is deſcribed at large, with a plate of the machine, in the Philoſophical Tranſactions for 1761, Vol. LII.

5. From the four experiments already related; firſt, of the mercury ſinking in the thermometer, by being expoſed to the ſtream of air from an air-gun; ſecondly, from its ſinking in the receiver of an air-pump, during the time of exhauſting it; thirdly, from its ſinking when expoſed to a ſtream of air from the air-veſſel of a water-engine; and, laſtly, from the curious phænomenon of ſnow and ice being produced by the ſtream of expanding air from the fountain of Hiero in an Hungarian mine; there is good reaſon to conclude, that in all circumſtances, when air is mechanically expanded, it becomes capable of attracting the fluid matter of heat from other bodies in contact with it.

Coldneſs of the Summits of Mountains.

Now, as the vaſt region of air which ſurrounds our globe is perpetually moving along its ſurface, climbing up the ſides of mountains, and deſcending into the vallies; as it paſſes along, it muſt be perpetually varying its degree of heat, according to the elevation of the country it traverſes: for in riſing to the ſummits of mountains it becomes expanded, having ſo much of the preſſure of the ſuper-incumbent air taken away, and, when thus expanded, it attracts or abſorbs heat from the mountains in contiguity with it; and when it deſcends into the valley, and is again compreſſed into leſs compaſs, it again gives out the heat it has acquired to the bodies it becomes in contact with.

The ſame thing muſt happen in reſpect to the higher regions of the atmoſphere, which are regions of perpetual froſt, as was always ſuſpected, and has of late been demonſtrated by the aërial navigators. When large diſtricts of air from the lower parts of the atmoſphere are raiſed two or three miles high, they become ſo much expanded by the great diminution of the preſſure over them, and thence become ſo cold, that hail or ſnow is produced from the precipitated vapour, if they contain any: and as there is, in theſe high provinces of the atmoſphere, nothing elſe for the expanded air to acquire heat from, after the precipitation of its vapour, the ſame degree of cold continues, till the air, on deſcending to the earth, acquires again its former ſtate of condenſation and of warmth.

The Andes, almoſt under the line, reſts its baſe on burning ſands; about its middle height is a moſt pleaſant and temperate climate, covering an extenſive plain, on which is built the city of Quito; while its forehead is incircled with eternal ſnow, coeval perhaps with the elevation of the mountain: yet, according to the accounts of ULLOA, theſe three diſcordant climates ſeldom intrench much upon each other's territories. The hot winds below, if they aſcend, become cooled by their expanſion, and hence cannot affect the ſnow upon the ſummit; and the cold winds, that ſweep the ſummit, become condenſed as they deſcend, and of temperate warmth, before they reach the fertile plains of Quito.

Correſpondence of the Heat of the Atmoſphere with the Height of the Barometer.

From this principle ſome of the ſudden changes of our atmoſphere from hot to cold, aud from dry to moiſt, may likewiſe be accounted for. During the laſt year I frequently obſerved, that when the barometer roſe (the wind continuing in the ſame quarter, *viz.* N.E. or S.W.) the air became many degrees warmer. A ſimilar fact is related from MUSSCHENBROEK, in Mr. KIRWAN's ingenious work on the Temperature of different Latitudes; *viz.* that in winter, when the mercury in the barometer deſcends, the cold increaſes. More accurate obſervations on this ſubject, when the air is ſtationary, or when the wind continues in the ſame quarter, might lead to the diſcovery of the quantity of heat ſqueezed out of the air by a certain preſſure.

The Devaporation of aerial Moiſture.

As heat appears to be the principal cauſe of evaporation, as well as of ſolution, and of fluidity in general, the privation of heat may be eſteemed the principal cauſe of devaporation: for though the air may, by its own power of attraction, or by means of the electricity it may contain, diſſolve and ſuſpend a portion of water, as water diſſolves and ſuſpends a portion of ſalt; yet, by the application of cold, theſe are reſpectively precipitated; and therefore heat may be aſſumed as the immediate cauſe of theſe ſolutions. Add to this, that water boils *in vacuo* with leſs heat; that is, it evaporates *in vacuo* faſter or eaſier than in the open air, and therefore the attractive power of the atmoſphere does not ſeem neceſſary to evaporation.

Now, when the barometer ſinks (from whatever cauſe not yet underſtood this may happen) the lower ſtratum of air becomes expanded by its elaſticity, being releaſed from a part of the ſuper-incumbent preſſure, and, in conſequence of its expanſion, robs the vapour which it contains of its heat; whence that vapour becomes condenſed, and is precipitated in ſhowers, as is viſible in the receiver of an air-pump above mentioned.

There are, however, two other curious circumſtances belonging to the devaporation of water, which have not been perhaps much attended to.

Firſt, that the deduction of a ſmall quantity of heat from a cloud or province of vapour, compared with the quantity of heat which was neceſſary to raiſe that vapour from water, will devaporate the whole. This circumſtance is evident in the operation of common ſteam-engines, in which a ſmall jet of water, whoſe heat is often above 48 degrees, perpetually devaporates the ſteam raiſed by a comparatively very great quantity of heat under the boiler. This difficult problem is explicable from the principles before eſtabliſhed: if a ſmall part of a province of vapour be ſuddenly condenſed, a vacuity takes place, and the contiguous walls of vapour expand themſelves into this vacuity; and thus a large area of vapour, perhaps of many miles in circumference, becomes more or leſs expanded; by this expanſion cold is produced (that is, its capacity of receiving heat is increaſed), and the whole is devaporated.

This very circumſtance exactly takes place in the famous ſteam-engine of Meſſ. WATT and BOULTON; which, from the happy combination of chemical and mechanic power, may juſtly be eſteemed the firſt machine of human invention. In this excellent machine, after the cylinder is filled with ſteam, a communication is opened between this reſervoir of ſteam and a ſmall cell, which is kept cold by ſurrounding water, and free from air by an air-ſyringe adapted to it. What then happens? The corner of the ſteam in the cylinder next to this vacuum (with which it now communicates) ruſhes into it, and the whole ſteam in the cylinder is thus ſuddenly expanded, and inſtantly devaporated: whence the very quick reciprocations of the piſton; and that, though the cylinder itſelf is always kept as hot as boiling water, that is, as hot as the ſteam was previous to its devaporation.

Something very ſimilar to this is often ſeen at the commencement of thunder-ſtorms; a ſmall black cloud at firſt appears, in a few minutes the whole heaven is covered with condenſing vapour, and the accumulation or eſcape of electric matter ſeems to be rather the conſequence than the cauſe of this ſudden and general devaporation.

A ſecond curious circumſtance of aërial devaporation is, that when the particles of aqueous vapour begin to approach each other by the diminution of their heat, they do not generate water exactly in proportion to ſuch diminution of heat; but the condenſation proceeds further, and not only a greater quantity of water is produced, but alſo a quantity of heat is ſet at liberty along with this exceſs of devaporation, and the atmoſphere becomes warmer than before the beginning condenſation. This exceſs of devaporation beyond the cold which produced it, is probably owing to the acquired momentum of the aqueous particles towards each other at the beginning of their condenſation, which carries them ſtill nearer each other; and to the ſmall moleculæ at firſt formed, poſſeſſing a greater attractive power over the uncondenſed vapour in their vicinity, and thus preſſing out more of the latent or combined heat.

Concluſion.

1. When a ſmall portion of air, ſuppoſe a few acres, becomes ſuddenly contracted into a leſs compaſs, either by inci-

dental cold, or by any other cauſe not yet underſtood (as the combination of dephlogiſtic and inflammable gaſes), the air next in vicinity ſuddenly expands itſelf to occupy the vacuity; and by its expanſion produces cold and devaporates, and then becomes compreſſible into leſs ſpace than it occupied before it parted with its vapour. This then gives occaſion to the next circum-ambient portion of air to go through the ſame proceſs, that is, to expand, attract the heat from its vapours, devaporate, and then become compreſſible into leſs ſpace; and thus, from a ſmall and partial contraction or diminution of air, it ſeems poſſible to devaporate a great province.

2. The vapour of a great province of air being thus condenſed, would leave a great vacuity in that part of the atmoſphere, which would be ſupplied by winds ruſhing in on all ſides. Suppoſe this to happen to the north of our climate, a ſouth-weſt wind would be produced here, which is otherwiſe very difficult to underſtand: and if it ſhould ever be in the power of human ingenuity to govern the courſe of the winds, which probably depends on ſome very ſmall cauſes; by always keeping the under currents of air from the S.W. and the upper currents from the N.E. I ſuppoſe the produce and comfort of this part of the world would be doubled at leaſt to its inhabitants, and the diſcovery would thence be of greater utility than any that has yet occurred in the annals of mankind.

November 22, 1787.

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V. *Some Obſervations on the Heat of Wells and Springs in the Iſland of* Jamaica, *and on the Temperature of the Earth below the Surface in different Climates. By* John Hunter, *M. D. F. R. S.; communicated by the Hon.* Henry Cavendiſh, *F. R. S.*

Read December 20, 1787.

TO THE HON. MR. CAVENDISH.

SIR,

THE following obſervations on the heat of ſprings and wells, and their application towards determining the mean temperature of the earth in different climates, were ſuggeſted by you in ſome converſation on that ſubject, previous to my going to Jamaica in 1780. If you think them deſerving the attention of the Royal Society, I muſt beg the favour of you to lay them before that learned Body.

I have the honour to be, &c.

JOHN HUNTER.

Charles-ſtreet,
Dec. 11, 1787.

THE great difference between the temperature of the open air, and that of deep caverns or mines, has long been taken notice of, both as matter of curioſity and ſurprize. After thermometers were brought to a tolerable degree of perfection, and meteorological regiſters were kept with accuracy, it became a problem, to determine what the cauſe was of this difference between the heat of the air, and the heat of the earth; for it was ſoon found, that the temperature of mines and caverns did not depend upon any thing peculiar to them; but that a certain depth under ground, whether in a cave, a mine, or a well, was ſufficient to produce a very ſenſible difference in the heat. In obſervations of this kind, there was perhaps nothing more ſtriking, than that the heat in ſuch caves was nearly the ſame in ſummer and winter; and this even in changeable climates, that admitted of great variation between the extremes of heat in ſummer, and cold in winter. There is an example of this in the cave of the Royal Obſervatory at Paris. The explanations, which have been attempted of this phænomenon, have turned chiefly upon a ſuppoſition, that there was an internal ſource of heat in the earth itſelf, totally independent of the influence of the ſun *. M. DE MAIRAN has beſtowed much labour on this ſubject, and by obſervation and calculation is led to conclude, that of the 1026° of heat (by REAUMUR's ſcale), which he finds to be the heat of ſummer at Paris, 34°,02 only proceed from the ſun, and the remaining 991°,98 from the earth, by emanations of heat from the center †. The proportion therefore of heat derived from this latter ſource is to that of the ſun, as 29,16 to 1. It muſt be evident, that an hypotheſis of this kind, which renders the influence of the ſun of ſmall account, is directly contrary to the general

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* Vid. MARTINE's Eſſays, p. 319.

† Memoir. de l'Acad. des Sciences, An. 1719 et 1765.

experience and conviction of mankind. Without entering, however, into any diſcuſſion of the *data* from whence M. DE MAIRAN draws his concluſions, it will be more ſatisfactory to conſider what the effect of the operation of thoſe laws of heat, with which we are acquainted, would be.

And firſt, it is well known, that heat in all bodies has a tendency to diffuſe itſelf equally through every part of them, till they become of the ſame temperature. Again, bodies of a large maſs are both cooled and heated ſlowly. Beſides the maſs of matter, there are two other conſiderations of much importance in the ſlow or quick tranſmiſſion of heat through bodies; theſe are their different conducting powers, and their being in a ſtate of ſolidity or fluidity. The conducting powers of heat are well known to be very various in different bodies; nor are they hitherto reducible to any law, depending either upon the denſity, or chemical properties of matter. Metals of all kinds are good conductors of heat, while glaſs, an heavy, ſolid, homogeneous body, is an extremely bad conductor, even when a metallic calx enters largely into its compoſition, as in flint-glaſs. A ſtate of fluidity greatly promotes the diffuſion of heat; for a body in a fluid ſtate, by the particles moving readily among each other from their different denſities or other cauſes, mixes the warm and cold parts together, which occaſions a quick communication of heat. To apply theſe obſervations to the preſent ſubject; the ſurface of the earth being expoſed to the great heats of ſummer, and the colds of winter, or more properly the low degree of heat of winter, will receive a larger proportion of heat in the former ſeaſon, and a ſmaller in the latter; and being further of a large maſs, and of a porous and ſpongy ſubſtance, and therefore not quickly ſenſible to ſmall variations of heat, it will become of a mean temperature at a certain depth, between the heat of ſummer, and the cold of winter, provided it contain no internal ſource of heat within itſelf. This concluſion is ſtrictly agreeable to the experiments and obſervations hitherto made, in heating and cooling bodies, or in mixing portions of matter of the ſame kind of different temperatures*. Water, though in a large maſs, follows in ſome degree the heat and cold of our ſummer and winter, from the mobility of its parts occaſioning a more ſpeedy diffuſion of heat. Air is quickly ſuſceptible of heat, and from the expanſions produced in it, and conſequent motions in the whole maſs, the temperature is ſoon rendered uniform.

The changes in the heat of the air are what we have meaſured, and we are to be underſtood to ſpeak of them, when we talk of the temperature of ſummer and of winter. It may be aſked then, is the heat of the ſun firſt communicated to the air, and thereby to the earth? No, the air is ſuſceptible of a very ſmall degree of heat from the rays of the ſun paſſing through it; for it is well known, they produce no heat in a tranſparent medium, and conſequently, that the air is only ſo far heated as it differs from a medium that is perfectly tranſparent. The heat produced by the rays of the ſun bears a proportion to their number, their duration, and their falling more or leſs perpendicularly; and it takes place at the points where they ſtrike an opaque and non-reflecting ſurface. The ſurface of the earth may therefore be conſidered as the place, from whence the heat proceeds, which is communicated to the air above, and the earth below. That this is really the caſe is evident from the ſuperior degree of heat, produced by the action of the rays of the ſun upon an opaque body, which will often be heated to 150° (FAHRENHEIT), while the temperature of the air is not above 90° †. It may ſeem, therefore, that to meaſure the heat communicated to the earth, it ſhould be done at the ſurface, where the action of the rays immediately takes place. But though the heat be produced at the ſurface, it is communicated freely to the air as well as the earth; and though the apparent intenſity of heat be greater in the earth, from the rays of light acting for a longer time upon the ſame parts of matter, yet there is little doubt that much the greater part is carried off by the air, which as it is heated flies off, and allows a freſh portion of cold air to come in contact with the heated ſurface. But ſtill it is immaterial, whether the heat of the ſun be excited more in the earth or in the air; for whichever has the larger proportion will in the end communicate a part to the other, and ſo reſtore the balance. The ſame obſervation applies to ſuch cauſes of cold as may operate at the ſurface of the earth, as evaporation, and that taken notice of by Mr. WILSON [*]. The air, therefore, near the ſurface of the earth will ſhew by a thermometer in the ſhade nearly, if not exactly, the ſame degree of heat that the ſun communicates to our terreſtrial globe; and if a mean of the heats thus ſhewn be taken for the year round, and we penetrate into the earth to that depth, that it is no longer affected either by the daily, monthly, or annual variations of heat, the temperature at ſuch depth ſhould be equal to the annual mean above mentioned. To aſcertain this with the utmoſt preciſion, it muſt be obvious, that numerous obſervations ſhould be made every day, correſponding to the frequent changes of temperature, which are known to happen in the courſe of the twenty-four hours in all climates; and upon theſe a daily mean ſhould be taken, and the annual mean deduced therefrom. This has not yet been done, but where we have obſervations from which a mean temperature can be deduced with any degree of certainty, it will be found not to differ greatly from the heat of deep caves, or wells in the ſame climate. If further experience and obſervation ſhould confirm the above opinions, it will be attended with this advantage, that we ſhall be poſſeſſed of an eaſy and ready method of aſcertaining the mean temperature of any climate; which, with a few obſervation's of the extremes of heat and cold at particular ſeaſons, will teach us as much of the country, with regard to heat and cold, as the meteorological obſervations of ſeveral years.

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* Vid. DE LUC Modifications de l'Atmoſphere, Vol. I. p. 285.

† MARTINE's Eſſays, p. 309.

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[*] Vid. Phil. Tranſ. Vol. LXX. p. 451. and Vol. LXXI. p. 386.

For obtaining the temperature of the earth the beſt obſervations are probably to be collected from wells of a conſiderable depth, and in which there is not much water. Springs iſſuing from the earth, although indicating the temperature of the ground from whence they proceed, are not ſo much to be depended upon as wells; for the courſe of the ſpring may be derived from high grounds in the neighbourhood, and it will thence be colder; it may run ſo near the ſurface as to be liable to variations of heat and cold from ſummer and winter; or it may be expoſed to local cauſes of heat in the bowels of the earth. Wells ſeem alſo better than deep caverns, for the apertures to ſuch are often large, and may admit enough of the external air to occaſion ſome change in their temperature. Wells are not, however, to be met with in all places, and in that caſe we muſt remain ſatisfied with the temperature of the ſprings.

The following obſervations were made in the Iſland of Jamaica, where there are flat lands in many parts towards the coaſt, but all the interior part of the country is mountainous. The heat is greateſt in the low lands, and decreaſes as you aſcend the mountains. The town of Kingſton is ſupplied with water from wells. The ground on which it ſtands riſes with a gentle aſcent as you recede from the ſea. In the low part of the town the wells are but a few feet deep, and many of them brackiſh. The heat of the water in ſome of them I have found as high as 82°; but they were evidently too near the ſurface not to be affected by the heat of the ſeaſons. As you aſcend, the wells are deeper, and the temperature is nearly 80° in all of them. What variations there are, come within one degree, that is, half a degree leſs than 80°, or half a degree more. They are of different depths, and ſome not leſs than 100 feet; though, after they are of half that depth, the temperature is nearly uniform. At the *Governor's Pen,* which is alſo in the low part of the country, a well, which is above 60 feet deep, is 79½°. There is a well at Half-way-Tree, 243 feet deep, which is 79°. Half-way-Tree is two miles from Kingſton, with a very gentle aſcent. Near Rock-Fort is a ſpring, immediately at the foot of the long mountain, which throws out a great body of water; the heat of it is 79°. All the places mentioned are but very little above the level of the ſea, probably not more than the depth of the wells at the reſpective places; for near Kingſton there are ſprings that appear juſt below the water-mark of the ſea, and thoſe that ſupply the wells are probably upon the ſame level.

The temperature of the air at Kingſton admits but of ſmall variation. The thermometer, at the hotteſt time of the day, and during the hotteſt ſeaſon of the year, ranges from 85° to 90°; in the cooleſt ſeaſon, and obſerved about ſun-riſe, which is the coldeſt time in the twenty-four hours, it ranges from 70° to 77°. I have ſeen it once as low as 69°, and two different times as high as 91°. The annual mean temperature cannot, therefore, either much exceed, or fall much ſhort of, 80°, as indicated by the wells.

The following ſprings were examined with much accuracy by the Hon. Mr. SEWELL, Attorney General of the Iſland.

Ayſcough's ſpring, on the road from Spaniſh Town to Puſey's, in St. John's pariſh, 75°.

Puſey's ſpring, ſtill higher in the mountains, 72°¾.

A ſpring near the baracks at Points Hill in St. John's pariſh, 70°.

The thermometer in the ſhade at Puſey's, during part of the month of June, was found to range from 69°½ to 79°½. It was obſerved both late at night, and early in the morning before ſun-riſe.

The ſpring in Brailsford Valley, about ten miles above Spaniſh Town, is 75°. The ſpring at Stoney Hill is 71°. Theſe were examined by Mr. HOME.

Mr. WALLEN's houſe, at Cold Spring, ſtands the higheſt of any in the iſland. By a meaſurement, ſaid to have been made by Mr. M^{c} FARLANE, it is reported to be 1400 yards above the level of the ſea. On the road to it, and about a mile below Mr. WALLEN's houſe, there is a ſpring that iſſues from the ſide of the hill, of the temperature of 65°. Cold Spring, which gives a name to the place, is about fifty feet below the houſe, and the heat of it is 61°¾. The thermometer in the ſhade at Mr. WALLEN's houſe, for ſome days in the month of April, ranged from 57° to 67°. It may be remarked, that the higher the ſprings the colder they are; and, as far as a conjecture can be formed from ſo few obſervations, they would appear not to differ much from the mean temperature of their reſpective places *.

It will not be out of place to add ſome obſervations made in England, relative to the ſame ſubject. The wells in and

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* The thermometers made uſe of were all made by Mr. RAMSDEN.

about London are either of no great depth, or are full of water, which are both conſiderable objections to their giving a mean temperature. The want of depth will make them ſubject to the variations of the ſeaſons; and a large quantity of water, even in a deep well, will take the temperature of the air more or leſs: for any change of temperature communicated at the ſurface will, from the fluidity of the water, be readily diffuſed through the whole. I ſuſpect it is owing to this cauſe, that the wells in the neighbourhood of Brighthelmſtone vary from 50° to 52°, for thoſe were the higheſt that had moſt water in them. My obſervations were made in ſummer. Theſe wells are of various depths, from 15 to 150 feet. That which I always found the coldeſt is not more than 22 feet deep; I never found its heat greater than 50°. It is near the beach, and is a tide well, that is, the water in it riſes and falls, and in ſo doing does not correſpond exactly with the tides, but follows them with an interval of about three hours. At the loweſt there is not more than a foot of water in it; and it may be conſidered as a ſubterraneous ſpring running through the bottom of the well. There are in fact numerous ſprings that break out upon the ſand, a few feet above the low-water mark, which are doubtleſs the ſame that ſupply the wells. As we are not acquainted with any cauſe that produces cold in the bowels of the earth, we muſt neceſſarily in every climate, conſider the loweſt degree of heat as approaching neareſt to the mean temperature; and therefore we cannot conclude the mean temperature at Brighthelmſtone to be more than 50°. The mean temperature of London is computed about 52°[*]; but Brighthelmſtone is nearly fifty miles farther ſouth than London, and is immediately upon the ſea, and muſt therefore be at leaſt as warm as London. It is evident, that the obſervations from which the mean is taken, muſt generally contain more of the extremes of heat than of cold, as the former happen in the day-time, and the latter in the night, in conſequence of which they will often eſcape notice. There is a table conſtructed by Dr. HEBERDEN *, expreſſing the heat in London for every month in the year, from a mean of ten years beginning with 1763, and ending with 1772. The mean temperature is given both at 8 A.M. and 2 P.M. There is further in the table, a column of the mean of the greateſt monthly colds in the night, obſerved during the ſame ten years by Lord CHARLES CAVENDISH, in Marlborough-ſtreet. There will not probably be any great error in conſidering the heat obſerved at 2 P.M. as the greateſt daily heat; and taking a mean btween the greateſt heats of the day, and greateſt colds of the night, they give 49°,196 for an annual mean, which is much lower than is commonly ſuppoſed. At the houſe of GEORGE GLENNY, Eſq. near Bromley, there is a well ſeventy-five feet deep, which I found in November 49°½. M. DE MAIRAN has given a table of the greateſt heats and greateſt colds obſerved at Paris for fifty-ſix years, beginning from 1701; and a mean of them is 10° above freezing, or 1010°, of REAUMUR's ſcale †. The temperature of the cave of the Obſervatory where thoſe obſervations were made, is 10°¼ above freezing, by the ſame ſcale of REAUMUR. There appears not therefore any neceſſity for an internal heat; on the contrary, it is matter of demonſtration, that were there any ſource of heat in the earth which was not equally in the air, the heat of the interior parts ought to be

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[*] KIRWAN's Temperature of different Latitudes, p. 73.

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* The Table alluded to follows this Paper.

† Mem. de l'Acad. des Sciences, An. 1765, p. 202.

higher than a mean: and did the *central heat* bear as high a proportion to that of the ſun as M. DE MAIRAN alledges, the heat of the earth itſelf ought to be a great deal above the mean temperature of the air, which from obſervation there is no ground for believing. It is eaſy to ſee the ſource of M. DE MAIRAN's error; he has founded his calculations upon the ſcale of REAUMUR, and conſiders the degrees of his thermometer as marking the real proportions, and abſolute quantity of heat *. It is a matter that cannot be denied, that we know nothing of the abſolute quantities of heat; and that the degrees of our thermometers are only to be conſidered as a few of the middle links of a chain, the length of which we are totally ignorant of, and therefore in no condition to compare its proportional parts. It deſerves, however, to be remarked, that obſervations of a late date have ſhewn, that the notions of cold upon which REAUMUR's ſcale was conſtructed, and upon which M. DE MAIRAN's calculations are founded, are imaginary and without foundation †.

Hot ſprings and volcanos may be produced as proofs of the exiſtence of an internal ſource of heat in the earth; but their operation appears to be limited to a very ſmall extent, and ſcarcely deſerving of notice in the preſent diſcuſſion. It is no uncommon thing to find ſprings of the uſual temperature cloſe by hot ſprings; and no volcano, with which we are yet acquainted, appears to have raiſed the temperature of the country immediately adjoining to it.

The ſea admits of change of temperature more quickly than the earth, particularly near the ſhore. The mean heat of the ſea at Brighthelmſtone, during the months of July, Auguſt, September, and October, was as follows:

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* Vid. Memoir. de l'Acad. des Sciences, An. 1765, p. 143.

† Vid. Phil. Tranſ. Vol. LXXIII. p. *303. 303. and 329.

July	$63^\circ\frac{1}{7}$
Auguſt	$63^\circ\frac{1}{2}$
September	58°
October	53°

The obſervations were made with a view to aſcertain the temperature of the ſea as a bath, and therefore the heat was taken about nine in the morning, and near the ſhore, the uſual time and place of bathing. The water gets hotter towards three o'clock in the afternoon, ſo that it not only follows the monthly, but even the daily changes of the temperature of the air. In the four months juſt mentioned, the extremes of heat and cold are conſiderable: I have ſeen it as hot as 71°, and as cold as 49°. In the month of Auguſt laſt, Sir HENRY ENGLEFIELD examined the heat of the ſea at the ſame time that I did, and we both found it 71°: it was about 4 P.M. of a very hot day. I may be allowed to remark, that ſea-bathing is a very different thing at different ſeaſons of the year, and requires an acquaintance with the variations of the temperature, to adapt it to particular caſes.

It were to be wiſhed, that the heat of wells and ſprings were examined at different ſeaſons of the year, in order to aſcertain the effect of ſummer and of winter upon them. The wells at New York are from 32 to 40 feet in depth, and Dr. NOOTH found them to have an annual variation of two degrees from 54° to 56°. There are few countries, in which the annual range of the thermometer is greater than at New York, and the neighbouring parts of America. In the ſummer it is often as high as 96°, and in winter it has been obſerved ſeveral degrees below the zero of FAHRENHEIT's ſcale.

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We may, I think, from all the obſervations we are yet in poſſeſſion of, conclude, that there is at preſent no ſource of heat in the earth, capable of affecting the temperature of a country, which is not derived from the ſun; and that the earth, whatever changes of temperature it may be conjectured to have undergone in former periods, is now reduced to a mean of the heat produced by the ſun in different ſeaſons, and in different climates.

VI. *A Table of the mean Heat of every Month for Ten Years in* London, *from* 1763 *to* 1772 *incluſively. By* William Heberden, *M. D. F. R. S. and A. S.*

Read January 31, 1788.

		At 8 A.M.	At 2 P.M.	Mean.	Night.
12	January	35°	39°	37°	34·7°
10	February	38	43	40·5	36.6
9	March	39	45	42	37·1
7	April	44	52	48	41 3
5	May	51	59	55	46.4
3	June	57	65	61	52.4
2	July	59	68	63.5	55·6
1	Auguſt	60	68	64	55·1
4	September	55	63	59	51·7
6	October	48	55	51·5	45·5
8	November	43	48	45·5	40
11	December	39	42	40·5	37·3

EXPLANATION OF THE TABLE.

The firſt column of figures denotes the order of the months according to their degrees of heat, beginning with Auguſt, in which the heat is greateſt.

The ſecond, and third, are the heats marked at the hour expreſſed at the top of each column, and the fourth is the mean between theſe two.

The laſt column is the mean of the greateſt cold at night, obſerved in Marlborough-ſtreet for twenty years, by the late Right Honourable Lord Charles Cavendish.

VII. *On Centripetal Forces. By* Edward Waring, *M. D. F. R. S. Profeſſor of Mathematics at* Cambridge.

Read January 10, 1788.

PROP. I.

1. LET a curve PpN (Tab. II. fig. 1.), of which the perpendiculars to the two neareſt points P and p of the curve are PO and pO, and conſequently O the center of a circle, which has the ſame curvature as the given curve in the point P; draw PY and ly tangents to the curve in the points P and p; from S draw Sy and ShY reſpectively perpendiculars to the tangents ly and PY; and let ShY cut the tangent ly in h; then will ultimately hY $(-\dot{P})$ be the decrement of the perpendicular $SY = P$; and the triangles lhY and POp be ſimilar: for the angles POp and hlY are equal, and the angles lYh and OPp right ones; therefore $PO : Pp :: lY$ ultimately $= PY : Yh$ decrement of the perpendicular, whence $Pp = \frac{Yh \times PO}{lY} = \frac{Yh \times PO}{PY}$.

1.2. Fig. 2. and 1. The force in the direction PS is as the ultimate ratio of $2 \times QR$ (the ſpace through which a body is drawn from the direction of its motion in the tangent in a given time towards the center of force); but ultimately $2QR = \frac{2QP^2}{PV}$, where QP is as the ſpace deſcribed in a given time, and conſequently as the velocity (V) of the body at the given point P, and PV the chord of curvature in the direction SP.

1.3. The increment (Pp) of the ſpace divided by the velocity V is ultimately as the increment of the time, and = the increment of the velocity ($\dot{V}$) divided by the force $\frac{2V^2}{PV} \times \frac{PY}{SP}$ in the direction of the tangent, that is, $\frac{Pp}{V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; for Pp ſubſtitute $\frac{Yb \times PO}{PY}$, and there reſults $\frac{-\dot{P} \times PO}{PY \times V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; and conſequently $\frac{-2\dot{P} \times PO}{SP \times PV} = \frac{\dot{V}}{V}$; but $\frac{SP \times PV}{2PO} = SY = P$, whence $\frac{-\dot{P}}{P} = \frac{\dot{V}}{V}$, and $V = \frac{a}{P}$, where a is an invariable quantity.

Cor. Since $V \times P$, that is, SY the perpendicular multiplied into the velocity (which is ultimately as Pp the ſpace deſcribed in a given time) is ultimately as the area deſcribed round the center S in a given time; but this rectangle $= a$, a given quantity; therefore the area, deſcribed round the center of force S in a given time, will be a given quantity, and thence in unequal times will be proportional to the times.

1.4. The ſagitta QR is ultimately as the force, when the time is given; and when the time is not given, it will be as the force into the ſquare of the time; from which expreſſion, by ſubſtituting for QR and the time their values, may be deduced ſeveral others.

Sir ISAAC NEWTON has demonſtrated this propoſition with the greateſt ſimplicity; and this is given to ſhew, that the ſame propoſition may be deduced from different principles.

PROP. II.

1. Fig. 3. Given the relation between SP′ the diſtance from a point S, and SY′ a perpendicular from the point S to P′Y, a line touching a curve in the point P′; to find the relation

between Sp' and Sy' (a perpendicular from the point S to $p'y$, a line touching the curve in the point p'); in which two curves PP′L and $pp'l$, the forces and velocities at any equal diſtances SP and Sp are equal, and conſequently the perpendiculars SY and Sy, at the above-mentioned equal diſtances SP and Sp are to each other in a given ratio N : n.

In the equation expreſſing the relation between SP′ and SY′ for SP′ and SY′ write reſpectively Sp and $\frac{Sy' \times N}{n}$, and there reſults the equation ſought: for the diſtances SP and Sp' being equal, the perpendiculars SY′ and Sy' are as N : n.

Ex. 1. Let S be the focus of a conic ſection, then will $\frac{1}{4} C^2 \times \frac{D}{T \pm D} = SY^2 = P^2$, where T and C denote its tranſverſe and conjugate axes, and D the diſtance SP; for P write $\frac{N}{n} \times p$, and there reſults the equation $\frac{1}{4} C^2 \times \frac{D}{T \pm D} = \frac{N^2}{n^2} \times p^2$, which is an equation to a conic ſection of the ſame name (*viz.* ellipſe, parabola, or hyperbola) as the given curve, of which the tranſverſe axis is T, and conjugate $= \frac{C \times n}{N}$, and perpendicular from the focus to the tangent $= p$. If T and C are infinite, and conſequently the curve a parabola, and the equation $\frac{1}{4} L \times D = P^2$, then will the *latus rectum* of the reſulting equation be $\frac{L \times n^2}{N^2}$.

Ex. 2. Let S be the center of the logarithmic ſpiral, then will the equation be $a \times SP = a \times D = SY = P$, and conſequently the reſulting equation $a \times D = \frac{N}{n} \times p$, whence $\frac{an}{N} \times D = p$ an equation to a logarithmic ſpiral having the ſame center.

Ex. 3. Let T and C be the ſemi-conjugate axes of a conic ſection, and S its center; then will the equation expreſſing the relation between the diſtance D and perpendicular P be $D^2 \pm \frac{T^2C^2}{P^2} = T^2 \pm C^2$; for P write as before $\frac{Np}{n}$, and there reſults the equation $D^2 \pm \frac{n^2T^2C^2}{N^2p^2} = T^2 \pm C^2$, an equation to a conic ſection of the ſame name, of which the tranſverſe and conjugate diameters are reſpectively two roots (x) of the equation $x^2 \pm \frac{n^2T^2C^2}{N^2x^2} = T^2 \pm C^2$, becauſe in this caſe $p = D$.

The ſum or difference of the ſquares of the tranſverſe and conjugate diameters, in all the reſulting equations, will be the ſame.

Cor. In every equal diſtance, the chord of curvature paſſing through the center of force is the ſame; for the forces in that direction, and the velocities at every equal altitude are the ſame.

PROP. III.

1. Fig. 4. and 3. Given an equation $A = o$, expreſſing the relation between the abſciſs $SM = x$ and ordinate $MP = y$; to find the equation expreſſing the relation between $SP = \sqrt{x^2 + y^2}$ and $SY = P$, the perpendicular from S on the tangent PY. From the equation $A = o$ find $\dot{x} = B\dot{y}$, which ſubſtitute for $\dot{x}$ in the equation $(\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} \times P = \dot{x}y \pm x\dot{y}$ deduced from the ſimilar triangles P*lo*, MTP, and STY, where $lo = \dot{x}$ and $Po = \dot{y}$; let the reſulting equation be $C = o$; reduce the three equations $A = o$, $C = o$, and $x^2 + y^2 = SP^2 = D^2$ into one, ſo that the unknown quantities x and y may be exterminated, and there reſults an equation expreſſing the relation between D and P.

Cor. Hence from the equation expreſſing the relation between x and y, the abſciſs and ordinate of a curve, can be deduced an equation expreſſing the relation between the diſtance SP and perpendicular SY; and from the equation expreſſing the relation between the diſtance SP and SY can be deduced an equation expreſſing the relation between the diſtance Sp and perpendicular Sy from the point S to the tangent py of a curve, whoſe force and velocity at every equal diſtance is the ſame as in the given curve, but the direction different.

2. Given an equation $K=o$ expreſſing the relation between $SP=D$ and $SY=P$; to find an equation expreſſing the relation between $SM=x$ and $PM=y$, the abſciſs and ordinate of the ſame curve.

In the given equation $K=o$ for D and P write reſpectively $\sqrt{x^2+y^2}$ and $\frac{y\dot{x}\pm x\dot{y}}{\sqrt{\dot{y}^2+\dot{x}^2}}$, and there reſults a fluxional equation $L=o$ of the firſt order, of which the fluent expreſſes the general relation between x and y.

Cor. If in the given equation for P be wrote nP', there reſults the equation $K'=o$, which expreſſes the relation between the perpendicular $Sy=P'$ and diſtance $Sp=D'$ of every curve, which at equal diſtances has the ſame velocity and force tending to S; reduce the equations $K'=o$, $D=\sqrt{x^2+y^2}$ and $nP'=\frac{y\dot{x}\pm x\dot{y}}{\sqrt{\dot{x}^2+\dot{y}^2}}$ into one, ſo that D and P′ may be exterminated, and there will reſult the ſame fluxional equation of the firſt order, expreſſing the relation between x, y, and their fluxions, whatever may be the value of n. The general fluent of this fluxional equation contains the relation between the abſciſs and ordinates of all curves, which have the ſame force and velocity at the ſame diſtance as the force and velocity in the given curve.

PROP. IV.

1. Let a body move in a given curve PH (fig. 5.), of which the velocity (v) at any point P is given: and let the forces f'', f''', &c. tending to all the given centers S'', S''', &c. (except two S and S') be given; to find the forces f and f' tending to the two points S and S'.

Draw a line PO perpendicular to the tangent yPy'; and from the given centers S, S', S'', &c. draw lines Sl and Sy, $S'l'$ and $S'y'$, $S''l''$ and $S''y''$, &c. perpendicular to the lines PO and yPy', &c.; then will $\frac{v^2}{PO} = f \times \frac{Pl}{PS} \pm f' \times \frac{Pl'}{PS'} \pm f'' \times \frac{Pl''}{PS''} \pm$ &c. (where PO is the radius of the circle having the ſame curvature as the curve in the point P), and $\frac{-v\dot{v}}{\dot{A}} = f \times \frac{Py}{PS} \pm f \times \frac{Py'}{PS} \pm$ &c. (where A denotes the arc of the curve PH); from the *data* may be deduced all the quantities contained in the above mentioned two equations, except f and f'; and conſequently from the two given ſimple equations be deduced the forces ſought f and f'.

2. Let the velocity of the body moving in the given curve be ſuppoſed always uniform; then $f \times \frac{Py}{PS} \pm f' \times \frac{Py'}{PS'} \pm f'' \times \frac{Py'}{PS''} \pm$ &c. $= o$.

Ex. Let the curve HPl be an ellipſe, and the two foci S and S' the centers of forces; then will $f \times \frac{Py}{SP} = f' \times \frac{Py'}{SP'}$, but the angle $SPy = S'Py'$, and conſequently $\frac{Py}{SP} = \frac{Py'}{S'P}$ and $f = f'$; but ſince $\frac{v^2}{PO} = f \times \frac{Sy}{SP} + f \times \frac{Sy'}{SP} = 2f \times \frac{Sy}{SP}$, and $v = a$, then will $f = \frac{a^2 \times SP}{2Sy \times PO}$ be the force tending to each focus.

In theſe and the ſubſequent caſes the lines Py, Py', Py'', &c. are to be taken negatively or affirmatively, as they are ſituated on the ſame or different ſides of P; and in the ſame manner the lines Pl, Pl', Pl'', &c. are to be taken negatively or affirmatively as they are ſituated on the ſame or different ſides of the tangent yPy', &c.

3. Let the centers M, M′, M″, M‴, &c. of forces be points not ſituated in the plane of the given curve HPI, &c. and the forces f''', f'''', &c. tending to each of the centers M‴, M⁗, &c. (except three M, M′, and M″) be given; to find the forces f, f', and f'' tending to thoſe three points M, M′, and M″.

Draw MS, M′S′, M″S″, &c. perpendicular to the plane HPI, &c. from the above-mentioned points, and aſſume the equation $f \times \frac{MS}{\sqrt{MS^2 + SP^2}} \pm f' \times \frac{M'S'}{\sqrt{M'S'^2 + S'P^2}} \pm f'' \times \frac{M''S''}{\sqrt{M''S''^2 + S''P^2}} \pm f''' \times \frac{M'''S'''}{\sqrt{M'''S'''^2 + S'''P^2}} + \&c. = c$, and the two preceding equations $\frac{v^2}{PO} = f \times \frac{Pl}{PM} \pm f' \times \frac{Pl'}{PM'} \pm f'' \times \frac{Pl''}{PM''} \pm \&c.$ and $\frac{-v\dot{v}}{A} = f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm \&c.$; from the *data* may be found all the quantities f''', f'''', &c.; and conſequently from the above mentioned equations may be deduced the forces f, f', and f''.

4. Let the body move in different planes, that is, in a curve of double curvature at the ſame points; draw PR a tangent to the curve at the point P, and PQ an arc of the curve of double curvature; draw alſo two planes PRV and PRT, cutting one another in the line PR; from the point Q let fall QV and QT perpendicular to thoſe planes reſpectively, and from the points V and T draw Vv and Tt reſpectively perpendicular to the line PR; let v be the velocity of a body moving in the given

curve at the point P, and aſſume $\frac{PQ^2}{Vv}=2C$ and $\frac{PQ^2}{Tt}=2C'$ reſpectively; from the given centers of forces M, M′, M″, M‴, &c. draw MS, MS′, M″S″, M‴S‴, &c.; M*s*, M′*s*′, M″*s*″, M‴*s*‴, &c. reſpectively perpendicular to the two planes RPV and RPT; and PL and P*l* perpendicular to the line PR in the ſame two planes RPV and RPT; and alſo SP, S′P, S″P, S‴P, &c; *s*P, *s*′P, *s*″P, &c.: from the points S, S′, S″, S‴, &c. *s*, *s*′, *s*″, &c. draw the lines SH, S′H′, S″H″, S‴H‴, &c. *sh*, *s*′*h*′, *s*″*h*″, *s*‴*h*‴, &c. reſpectively perpendicular to the lines PL and P*l*; and SK, S′K′, S″K″, S‴K‴, &c. *sk*, *s*′*k*′, *s*″*k*″, *s*‴*k*‴, &c. perpendicular to the line RP; and let the forces f''', f'''', &c. tending to all the points M‴, M⁗, &c. (except three, M, M′, and M″) be given; then from the three given equations $\frac{v^2}{C}=\frac{PH}{MP}\times f\pm\frac{PH'}{M'P}\times f'\pm\frac{PH''}{M''P}\times f''\pm$ &c. and $\frac{v^2}{C'}=\frac{Ph}{MP}\times f\pm\frac{Ph'}{M'P}\times f'\pm\frac{Ph''}{M''P}\times f''\pm$ &c. and $\frac{-v\dot{v}}{A:}=\frac{PK}{MP}\times f\pm\frac{PK'}{M'P}\times f'\pm\frac{PK''}{M''P}\times f''\pm\frac{PK'''}{M'''P}\times f'''\pm$ &c. $=\frac{Pk}{MP}\times f\pm\frac{Pk'}{M'P}\times f\pm\frac{Pk''}{M''P}\times f''\pm$ &c. which contain only three unknown quantities, can be deduced the forces f, f', and f'', required, tending to the points M, M′, and M″.

PROP. V.

Let a body acted on by forces tending to any given points S, S′, S″, &c. move in a given curve, to find its velocity in any point of the curve.

Find the fluent of the fluxion $(f\times\frac{Py}{PS}\pm f'\times\frac{Py'}{PS'}\pm f''\times\frac{Py''}{PS''}\pm f'''\times\frac{Py'''}{PS'''}\pm$ &c.) $\dot{A}=f\dot{D}\pm f'\dot{D}'\pm f''\dot{D}''\pm$ &c. $=-v\dot{v}$ when the forces are all contained in the ſame plane; or the fluent of

(*f* ×

$(f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm \&c.) \times \dot{A}$ (when contained in different planes) $= f \times \dot{PM} \pm f' \times \dot{PM'} \pm f'' \times \dot{PM''} \pm \&c. = f \times \dot{D} \pm f' \times \dot{D'} \pm f'' \times \dot{D''} \pm \&c.$; but ſince f, f', f'', &c. are given functions of the quantities D, D′, D″, &c. the fluents of $f \times \dot{D}$, $f' \times \dot{D'}$, $f'' \times \dot{D''}$, &c. can be found; which, when properly corrected will be as $\frac{v^2}{2} = \frac{1}{2}$ the ſquare of the velocity in any point P. A denotes the arc of the curve, and D, D′, D″, &c. the reſpective diſtances of the body from the centers of forces.

Cor. The increment of the time of deſcribing any arc of the above-mentioned curve will be as the increment of the arc $= \dot{A}$ divided by the velocity found above, and conſequently the time itſelf will be as the fluent of it properly corrected.

PROP. VI.

1. Let a body move in any curve, and be acted on by forces tending to any given points, S, S′, S″, S‴, &c.; all of which, except the force f tending to the point S, let be given, to find f the force tending to S.

Let Sy, S′y', S″y'', &c. be perpendicular to the tangent Py of the curve at the point P; reſolve the forces f, f', f'', f''', &c. tending to S, S′, S″, S‴, &c. reſpectively into two forces, of which one acts perpendicular to Py, the other, Sl, S′l', S″l'', &c. perpendicular to PO, which is perpendicular to Py; let PO be radius of the circle of the ſame curvature as the curve, and v the velocity of the body at the point P; then will $\frac{v^2}{PO} = f \times \frac{Sy}{SP} \pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm f''' \times \frac{S'''P}{S'''y'''} \pm \&c.$ and $-\frac{v\dot{v}}{\dot{A}} = f \times \frac{Py}{SP} \pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P} \pm f''' \times \frac{Py'''}{S'''P} \pm \&c.$: for $\frac{Sy \times PO}{SP} =$

½ chord of the circle of curvature, which paſſes through S, write C; and for $PO \times (\pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm \&c.)$ ſubſtitute H, and for $\frac{Py}{SP}$ write B; and for $\pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P} + \&c.$ ſubſtitute D, and the two preceding equations become $v^2 = f \times C + H$ and $-v\dot{v} = (Bf + D)\dot{A}$, where $\dot{A}$ denotes as before the increment of the arc of the curve: from the firſt equation $v\dot{v} = \frac{f \times \dot{C} + C\dot{f} + \dot{H}}{2} = -(Bf\dot{A} + D\dot{A})$ and conſequently $C\dot{f} + (\dot{C} + 2B\dot{A})f + \dot{H} + 2D\dot{A} = o$, from which fluxional equation may be deduced the force f tending to the center (S) $= -C^{-1} \times e^{-\int \frac{2B\dot{A}}{C}} \times \int (\dot{H} + 2D\dot{A}) \times e^{\int \frac{2B\dot{A}}{C}}$, where e is the number, whoſe hyper. log. = 1.

Cor. Fig. 1. Let f', f'', f''', &c. be each $= o$, then will $D = o$, $H = o$, and conſequently $\int (2\dot{D}A + \dot{H}) \times e^{\int \frac{2B\dot{A}}{C}} =$ conſt $= a$, and $f = -a \times C^{-1} \times e^{-\int \frac{2B\dot{A}}{C} = \frac{2B \times S\dot{y} \times PO}{C \times Py} = \frac{2S\dot{y}}{Sy}} = -aC^{-1} \times e^{-\int \frac{2S\dot{y}}{Sy}}$; whence $f = \frac{-a}{Sy^2 \times C}$ as is generally known, where a denotes an invariable quantity.

Cor. The force f being found, the ſquare of the velocity may be deduced from the equation $v^2 = f \times C + H$, and the time from the fluent of the fluxion $\frac{\dot{A}}{v} = \frac{\dot{A}}{\sqrt{f \times C + H}}$.

2. Let the body move in a curve of double curvature, and let the forces f'', f''', &c. tending to all the points M″, M‴, &c. (except two, M and M′) be given; to find the forces tending to the points M and M′.

Aſſume the three equations before given in Prop. 4. *viz.* $\frac{v^2}{C} = \frac{PH}{MP} \times f \pm \frac{PH'}{M'P} \times f' \pm \frac{PH''}{M''P} \times f'' \pm$ &c. $\frac{v^2}{C'} = \frac{Pb}{MP} \times f \pm \frac{Pb'}{M'P} \times f \pm \frac{Pb''}{M''P} \times f'' \pm$ &c. and $-v\dot{v} = \left(\frac{PK}{MP} \times f \pm \frac{PK'}{M'P} \times f' \pm \frac{PK''}{M''P} \times f'' \pm \right.$ &c. $= \frac{Pk}{MP} \times \dot{f} \pm \frac{Pk'}{M'P} \times f' \pm$ &c.$\left.\right) \times \dot{A}$, from the two former may be deduced the equations $v\dot{v} = \alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma}$, and $v\dot{v} = \alpha'\dot{f} + \beta'\dot{f}' + f\dot{\alpha} + f'\dot{\beta}' + \dot{\gamma}'$, where $\alpha = \frac{C \times PH}{2MP}$, $\beta = \pm \frac{C \times PH'}{2M'P}$, $\gamma = \pm \frac{1}{2} C \left(\frac{PH'' \times f''}{M''P} \pm \frac{PH''' \times f'''}{M'''P} \pm \right.$ &c.$\left.\right)$; $\alpha' = \frac{C' \times Pb}{2MP}$, $\beta' = \pm \frac{C' \times Pb'}{2M'P}$, $\gamma' = \pm \frac{1}{2} C' \left(\frac{Pb'' \times f''}{M''P''} \pm \frac{Pb''' \times f'''}{M'''P} \pm \right.$&c.$\left.\right)$, whence may be derived the two equations $\alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma} = \alpha'\dot{f} + \dot{f}' + f\dot{\alpha}' + f'\dot{\beta}' + \dot{\gamma}' = \pi f \pm \rho f' \pm \sigma$, where $\pi = -\left(\frac{PK}{MP} = \frac{Pk}{MP}\right) \times \dot{A}$, $\rho = \pm \left(\frac{PK'}{M'P} = \frac{Pk'}{M'P}\right) \times \dot{A}$, $\sigma = -\left(\pm \frac{PK''}{M''P} \times f'' \pm \frac{PK'''}{M'''P} \times f''' \pm \right.$&c. $=$ &c.$\left.\right) \times \dot{A}$.

Reduce theſe two equations to one, ſo that f', f'', &c. and their fluxions, may be exterminated, and there reſults a fluxional equation of the formula $H\ddot{f} + K\dot{f} + Lf + M = o$, where H, K, L, and M, are functions of one of the before mentioned variable quantities (for example, MP = W) which may be ſuppoſed to flow uniformly, and its fluxion.

PROP. VII.

1. Fig. 6. Given the force tending to any point S, the velocity and direction of the body; to find the curve deſcribed.

Let the body acted on by a force f tending to S, at the diſtance D′ from S be projected in the direction P′Y′, with a velocity $\dot{H}$; and let the perpendicular from S to the tangent P′Y′ be A; from the general fluent of $f \times \dot{D}$, where D denotes the diſtance from S, and f is a function of D, properly corrected find its velocity V at diſtance D, and conſequently the perpendicular SY from the center S to the tangent PY at diſtance $D = SP$, which will be $\frac{A \times H}{V} = SY$; but A and H are given quantities, and V a known function of D; therefore SY and $\sqrt{SP^2(D^2) - SY^2} = PY$ will be known functions of D; and from the ſimilar triangles SPY and PQT may be deduced $PY : SY :: PT = \dot{D} : QT$, and conſequently $SP \times QT = D \times \frac{\dot{D} \times SY}{PY}$ (which is a known function of D multiplied into $\dot{D}$) will be as the increment of the area deſcribed round the center of force, of which the fluent properly corrected is proportional to the area deſcribed round the center of force, and conſequently to the time. In like manner, $\frac{Sy \times \dot{D}}{D \times PY} = \frac{QT}{SP}$ (proportional to the increment of the angle deſcribed by the body round S) is a function of D multiplied into $\dot{D}$, of which the fluent properly corrected, or angle, will be as a function of D.

1.2. Fig. 7. Given the above-mentioned force, &c.; to find an equation expreſſing the relation between the abſciſs $SM = x$ and ordinate $MP = y$ of the curve deſcribed, and their fluxions.

From the ſimilar triangles P*po* and LPM can be deduced $po = \dot{y} : oP = \dot{x} :: PM = y : LM = \frac{y\dot{x}}{\dot{y}}$; but $LM \pm SM = \frac{y\dot{x}}{\dot{y}} \pm x = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} = LS$; and conſequently $Pp = \sqrt{\dot{x}^2 + \dot{y}^2} : po = \dot{y} :: LS = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} : SY = \frac{y\dot{x} \pm x\dot{y}}{\sqrt{\dot{x}^2 + \dot{y}^2}}$; but SY is a function to be deduced as above of $SP=\sqrt{x^2+y^2}$, whence the fluxional equation $\frac{y\dot{x}\pm x\dot{y}}{\sqrt{x^2+y^2}}=\varphi:(x^2+y^2)$.

2. Fig. 8. Let a body be acted on by any number of forces (f, f', f'', f''', &c.) in the ſame plane tending to the given points S, S′, S″, S‴, &c.; to find an equation expreſſing the relation between $SP=D$ and $S'P=D'$, and their fluxions, where P is a point ſituated in the curve which the body deſcribes.

Suppoſe YP a tangent to the curve at the point P, and PZ perpendicular to it; and reſolve all the forces tending to S, S′, S″, &c. reſpectively into two others; one in the direction PY, and the other in the direction PZ; ſubſtitute for SP, S′P, S″P, S‴P, &c. reſpectively D, D′, D″, D‴, &c.; and ſuppoſe SY, S′Y′, S″Y″, S‴Y‴, &c. perpendicular to the line PY; then will the triangles PQT and SPY, PQ′T′ and S′PY′ be ſimilar, where PQ denotes a very ſmall arc, and QT and QT′ are perpendicular to the lines SP and S′P; hence $PQ=\frac{PT\times SP}{PY}=\frac{\dot{D}\times D}{PY}=\frac{PT'\times S'P}{PY'}=\frac{\dot{D}'\times D'}{PY'}$; and conſequently $PY:PY'::\dot{D}\times D:\dot{D}'\times D'$; and if the quantities D, D′, Ḋ and Ḋ′ are given, the ratio of PY : PY′ will be given; which being given, together with the line $SS'=a$, the lines PY and PY′, SY and S′Y′, can be found; for, drawing SL parallel to PY, and meeting S′Y′ in L, let $PY'=m\times PY$, then $YY'=(m\pm 1)\ PY=SL$, $SY=\sqrt{(SP^2-PY^2)}=\sqrt{(D^2-PY^2)}$, $S'Y'=\sqrt{(S'P^2-PY'^2)}=(D'^2-m^2\times PY^2)$, $LS'=S'Y'\pm SY=\pm\sqrt{(D'^2-m^2PY^2)}\pm\sqrt{(D^2-PY^2)}$; and $SS'^2=SL^2+LS'^2$ an equation in which all quantities (except PY) are given, and conſequently PY is determined by an equation, which will be a quadratic; but PY being found, from thence PY′, SY and

S′Y′ may be deduced, which are conſequently all functions of D, D′, $\dot{D}$, $\dot{D}'$, and invariable quantities; and their fluxions $P\dot{Y}'$, $S\dot{Y}$, and $S'\dot{Y}'$ functions of D, D′, $\dot{D}$ $\dot{D}'$, $\ddot{D}$, and $\ddot{D}'$: from the ſimilar triangles before given $S\dot{Y} : PQ = \frac{D\dot{D}}{PY} :: PY : \frac{D\dot{D}}{S\dot{Y}} = PO$ the radius of curvature hence PO is a function of D, D′, $\dot{D}$, $\dot{D}'$, and $\ddot{D}'$, if $\ddot{D} = o$; and from D, D′, SS′, $\dot{D}$, $\dot{D}'$, and the point S″ given in poſition can be determined S″P, S″Y″ and PY″; for let $S''b = C$ be drawn perpendicular to $SS' = a$, and $Sb = b$; then will Sl (if Pl be a perpendicular from the point P to the line SS′) $= \pm \frac{a^2 + D^2 - D'^2}{2a}$, and $S'l = \pm \frac{a^2 + D'^2 - D^2}{2a}$, and $Pl = \surd(SP^2 - Sl^2)$, and $S''P = \surd((b \pm Sl)^2 + (C \pm Pl)^2)$; draw S″Y″ perpendicular to the tangent PY, and cutting the lines SS′ and SK parallel to PY in o and n reſpectively; then will $ob = \frac{C \times \surd(SS'^2 - (PY \pm PY')^2)}{(PY \pm PY') = l}$, $S''o = \frac{C \times SS'}{l}$; (and from the ſimilar triangles $S''ob$ and Son) $on = (b \pm ob) \times \frac{ob}{S''o}$; whence $S''Y'' = \pm S''o \pm on \pm SY$ will be a known function of D, D′, $\dot{D}$ and $\dot{D}'$, and invariable quantities: the ſame may be predicated of ſimilar lines drawn to the centers S‴, S⁗, &c.; and conſequently $(f \times \frac{PY}{SP} \pm f' \times \frac{PY'}{S'P} \pm f'' \times \frac{PY''}{S''P} \pm f''' \times \frac{PY'''}{S'''P} \pm$ &c.$) \times \dot{A}$ (where $\dot{A}$, as before, denotes the fluxion of the arc of the curve) $= f \times \dot{D} \pm f' \times \dot{D}' \pm f'' \times \dot{D}'' \pm f''' \times \dot{D}''' \pm$ &c. $= -v\dot{v}$, if v denotes the velocity; but as f, f', f'', f''', &c. are functions of D, D′, D″, D‴, &c. reſpectively, the fluent of the above mentioned quantity $f\dot{D} \pm f'\dot{D}' \pm f''\dot{D}'' \pm$ &c. can be found in terms of D, D′, D″,

D'', D''', &c. from the fluents of the fluxions $f\dot{D}$, $f'\dot{D}'$, &c.; and conſequently in terms of D and D', which let be Z, then will $Z = \frac{-v^2}{2}$; but $v^2 = -2Z = PO \times (f \times \frac{SY}{SP} \pm f' \times \frac{S'Y'}{S'P} \pm f'' \times \frac{S''Y''}{S''P} \pm f''' \times \frac{S'''Y'''}{S'''P} \pm \&c.)$ a fluxional equation of the ſecond order expreſſing the relation between D and D', and their fluxions.

2. To find an equation expreſſing the relation between $x = SM$ and $y = MP$, where SM (x) is the abſciſs beginning from S and continued in the line SS', and MP (y) the perpendicular ordinate of the curve deſcribed by a body acted on by the above mentioned forces: in the fluxional equation found before for D and D' and their fluxions ſubſtitute $(x^2 + y^2)^{\frac{1}{2}}$ and $((SS' \pm x)^2 + y^2)^{\frac{1}{2}}$ and their fluxions, and there reſults the equation ſought.

Cor. It eaſily appears, that the general fluent may contain two invariable quantities to be aſſumed at will, or according to the conditions of the problem; that is, at a given diſtance the velocity and the direction may be aſſumed at will, and conſequently the general fluxional equation expreſſing the above mentioned relation will be of the ſecond order, if no fluents are contained in it.

Cor. From Py and Py', and the points S and S' being given, can eaſily be deduced geometrically the direction of the tangent and the lines Sy, Sy', &c.; for divide the line SS' in r, ſo that $Py \pm Py' : SS' :: Py : Sr$, and through r draw the line Pr, the perpendicular to Pr through P will be the tangent yPy'; to this line the perpendiculars from S and S' will be the lines Sy and $S'y'$ required.

Cor. From the fluent of the above-mentioned fluxional equation may be deduced the velocity V in terms of D and D′; and from the fluent of $\frac{D\times\dot{D}}{Py\times V}$, which is a function of D multiplied into $\dot{D}$, may be deduced the time.

3. If the plane in which the body (P) moves, and all the forces f', f'', f''', &c. tending to points M′, M″, M‴, &c. not ſituated in the ſame plane (except one f tending to a given point M) be given, then the force tending to that point can be found, and the curve deſcribed. Reſolve all the forces tending to the points M, M′, M″, M‴, &c. into two others; one MS, M′S, M″S, M‴S, &c. perpendicular to the plane in which the body moves, and the other SP, S′P, S″P, S‴P, &c. in the plane; then will $f\times\frac{MS}{MP}\pm f'\times\frac{M'S'}{M'P}\pm f''\times\frac{M''S''}{M''P}\pm \&c.=0$, from which equation f the force tending to the point M may be found; then, from the preceding propoſition find the curve, which a body agitated by forces $f\times\frac{SP}{MP}$, $f'\times\frac{S'P}{M'P}$, $f''\times\frac{S''P}{M''P}$, &c. tending to the points S, S′, S″, &c. deſcribes, and it will be the curve required.

4. If the body moves in a curve of double curvature, and the forces f, f', f'', &c. tending to all the centers M, M′, M″, M‴, &c. be given; from the fluent of the fluxional quantity $(f\times\frac{Py}{MP}\pm f'\times\frac{Py'}{M'P}\pm f''\times\frac{Py''}{M''P}\pm f'''\times\frac{Py'''}{M'''P}\ \&c)\times\dot{A}$ (A denoting the ſame quantity as before) $=f\times M\dot{P}\pm f'\times M'\dot{P}\pm f''\times M''\dot{P}\pm f'''\times M'''\dot{P},\pm \&c. =f\times\dot{D}\pm f'\times\dot{D}'\pm f''\times\dot{D}''\pm f'''\times\dot{D}'''\pm \&c. =\dot{Z}=-v\dot{v}$ (f, f', f'', f''', &c. being given functions of D, D′, D″, &c. reſpectively) can be deduced the ſquare of the velocity $=-2z$, which will be a function of

D, D', D'', D''', D'''', &c., and conſequently a function of D, D', D'', eaſily to be derived: ſubſtitute this function $-2z$ for v^2 in the two following equations $\frac{v^2}{R'}=F'$ and $\frac{v^2}{R''}=F''$, where R' and R'' denote the radii of curvature in two different planes of which the tangent above mentioned in Prob. 4. art. 4. is their interſection, and F' and F'' the ſum of the forces in lines perpendicular to the tangent, and in the reſpective planes: from theſe forces, calculated in terms of the diſtances from three given points D, D', and D''; or in terms of two abſciſſæ and one ordinate, and from the radii R' and R'' may be deduced two fluxional equations of the ſecond order, expreſſing the relation between three diſtances D, D', and D'', &c. which may always be reduced to one fluxional equation of the fourth order expreſſing the relation between one abſciſs and its correſpondent ordinates, or the diſtances from two given points.

5. The general fluxional equation expreſſing the relation between the diſtances from two given points will be of the fourth order, if no fluents are contained in it; for it admits of four different quantities to be aſſumed at will, or according to the conditions of the problem.

6. If ſome points, to which the forces tend, are ſituated at an infinite diſtance; that is, ſome forces always act parallel to themſelves; from the given forces acting either to given points, or in parallel directions, by the equation $f\times\dot{D}\pm f'\times\dot{D}'\pm f''\times\dot{D}''\pm\text{\&c.}=-v\dot{v}$ can be deduced the ſquare of the velocity at a point P in terms of the diſtances from two given points, or of an abſciſs and ordinate; if the centers, &c. and parallel forces are all ſituated in the ſame plane: or in terms of the diſtances from three points, or two abſciſſæ and an ordi-

nate, if ſituated in different planes; from the centers, &c. and forces given, find the ſum F of the forces in any direction (PL) (the direction of the tangent excepted) acting on the body at the point P, and the chord of curvature C of the curve at the ſame point and in the ſame direction; in the equation $v^2 = \frac{1}{2} F \times C$ for v^2 ſubſtitute the value found before, and there reſults an equation expreſſing the relation between the diſtances from two points, or an abſciſs and ordinate, &c. if the forces act in the ſame plane: but if the forces act in different planes, find the ſum F and F′ of the forces at the point P in directions which are not both ſituated in one plane with the tangent and each other; and alſo the chords C and C′ of curvature in thoſe directions in terms of the diſtances from three points, or two abſciſſæ and one ordinate, &c. In the equations $v^2 = \frac{1}{2} F \times C$ and $v^2 = \frac{1}{2} F' \times C'$ for v^2 ſubſtitute its value found from the principles before given; and there reſult two fluxional equations of the ſecond order expreſſing the relation between the diſtances from three points, or two abſciſſæ and an ordinate, &c.

PROP. VIII.

Fig. 9. Let a body move in a curve P*p*, &c. and be acted on at P′ by a force *f* (which is as any function of the diſtance SP′) tending to S; let the velocities at P and *p* be repreſented by the lines YP and *yp* in the direction of the tangents to the points P and *p*; reſolve theſe forces YP and *yp* into two others Y*k* and *k*P, and *yl* and *lp*, of which one *k*Y and *yl* is parallel to the line SL; the other *k*P and *lp* is parallel to MP: let a body fall in the right line LS, and the force acting on the body at M′ be to the force acting on the body moving in the curve at P′ :: SM′ : SP′, and P′M′, PM

and *pm* be perpendicular to SL; then, if the velocity of the body falling in the right line SL at the point M be *k*Y, the velocity of the body at the point *m* acted on by the above mentioned forces will be *yl*.

This is eaſily demonſtrated from the reſolution of forces,

2. Through S draw SN parallel to PM or *pm*, &c., and aſſume in the line (SN) $SP = PM$ and $Sp = pm$, and let the force at P′ in the line SN and diſtance $= M'P'$: the force of the body moving in the curve at the diſtance $P'S : P'M' : SP'$; then if the velocity at the diſtance $SP = PM$ be P*k*, the velocity at the diſtance $Sp = pm$ will be *pl*.

Cor. The force in the direction of the line SL vaniſhes in the point where a perpendicular SN to the line SL paſſing through the point S cuts the curve, and conſequently the velocity in the direction of SL in that point is the greateſt or leaſt, &c.; but if the tangent of the curve be perpendicular in any point to LS, then the velocity in the direction LS is nothing: the ſame may be applied to the velocity in any other direction.

Ex. Fig. 10. Let a body move in the circumference of a circle SPA, of which the center of force is a point S in the circumference; it is known, that the force in the direction and at the diſtance SP is as SP^{-5}; but the force in the direction SP is by the hypotheſis to the force in the direction (SA) :: $SP : SM$, if PM be perpendicular to SM, and conſequently the force in the direction (SA) is as $SM \times SP^{-6}$; but if AS be a diameter, $AS \times SM = SP^2$; therefore $SM \times SP^{-6} = SM \times AS^{-3} \times SM^{-3} = \frac{SM^{-2}}{AS^3}$; and the diameter AS being given, the force in the line SA varies as SM^{-2}, that is, inverſely as the ſquare of the diſtance: if the force varies as $SM^{-2} = x^{-2}$, then $v\dot{v}$ will vary as

$-\frac{\dot{x}}{x^2}$, where v denotes the velocity; and v^2 will vary as $\frac{1}{x}-\frac{1}{SA}$, which agrees with the ſquare of the velocity deduced from the preceding principles; for $v = PY$ the velocity at P is inverſely as the perpendicular $SY = SM$ let fall from the center of force on the tangent; but $SA^2 : 2SP \times PA ::$ velocity PY as $\frac{1}{SY}=\frac{1}{SM}$: Pl the velocity at M; whence Pl^2 (the ſquare of the velocity at M) $= \frac{4SP^2 \times PA^2}{SA^4} \times PY^2$ which varies as $\frac{4SP^2 \times PA^2}{SA^4} \times \frac{1}{SM^2} = \frac{4PA^2}{SA^3 \times SM} = \frac{4SA^2 - 4SA \times x}{SA^3 \times x}$, and conſequently as $\frac{1}{x}-\frac{1}{SA}$ the ſame as above.

2. Fig. 9. If any number of forces act on a body at P in any given directions parallel, or tending to given points; reſolve all the forces into two others; one in a given direction SM, and the other in a direction PM perpendicular to it, of which let F be the ſum of the forces reſulting in the direction M*m*S, and *f* the ſum of the forces reſulting in the direction PM; reſolve the velocity V of the body at P, which is in the the direction of the tangent PY, into two others V' and V'', one in the direction parallel to the line SM, and the other perpendicular to it: in the ſame manner reſolve the velocity v of the body at p, which is in the direction of the tangent py, into two others v' and v'', one in the direction parallel to the line SM, and the other perpendicular to it: then if the velocity of the body moving in the right line SM at M be V', and it is conſtantly acted on by a force $= F$, the velocity of the body at m will be v': and if the body move from P in a direction perpendicular to SM with a velocity as V'', and be always acted on by a force f, the velocity at the diſtance $PM - pm$ will be v''.

Cor. From the forces given and the velocities in the above mentioned directions at the point P, can be deduced the velocities in the ſame directions at the point p, and conſequently the tangent to the curve at the point p.

PROP. IX.

1. Let the reſiſtance of a body, moving in a right line, be as any function V of the velocity v; then will $\dot{t} = \frac{\dot{v}}{V}$, $\dot{x} = \frac{-v\dot{v}}{V}$; where $\dot{t}$, $\dot{v}$, and $\dot{x}$, denote the increments of time, velocity, and ſpace; their fluents properly corrected will give the time and ſpace in terms of the velocity.

2. Let a body move in a right line, and be acted on by an accelerating force in that line, which varies as any function X of the diſtance x from a given point; and reſiſted by a force which is as any function V of the velocity v into its denſity X′, which varies alſo as a function of x and v; then will $(X + aVX')\dot{x} = -v\dot{v}$, from its fluent x can be found in terms of v, or v in terms of x; and thence $\dot{t} = \frac{\dot{v}}{X + aVX'}$, of which the fluent properly corrected gives the time.

Ex. 1. Let $V = v^2$ and X′ a function of x; that is, let the reſiſtance be as the ſquare of the velocity and denſity, whence $(X + av^2X')\dot{x} = -v\dot{v}$, of which equation the fluential will be

$$e^{\int 2aX'\dot{x}} \times \frac{v^2}{2} = -\int e^{\int 2aX'\dot{x}} \times X\dot{x} + A, \text{ and } t =$$

$$\int \frac{\dot{x}}{\surd - (e^{-\int 2aX'\dot{x}} \times (\int e^{\int 2aX\dot{x}} \times X\dot{x} + A))} + B,$$ where A and B are invariable quantities to be aſſumed according to the conditions of the problem.

1.2. Let $e^x = X'$ and $X = b$, which is ſuppoſed to correſpond nearly to the ſtate of our atmoſphere, then will $v^2 = -2 \times e^{-\int 2aX'\dot{x}} \times \int e^{\int 2aX'\dot{x}} \times X\dot{x} = -2 \times e^{-\int 2ae^x\dot{x}} \int e^{\int 2ae^x\dot{x}} \times b\dot{x} = -2e^{-2ae^x - b} \times \left(\int e^{2ae^x + b} \times b\dot{x} + A\right)$, e being the number, whoſe hyperbolic log. is 1, and b and A quantities to be aſſumed according to the conditions of the problem.

1.3. Let $X = X'$, and it becomes $X\dot{x} = \frac{-v\dot{v}}{1+aV}$, and $\dot{t} = \frac{-\dot{v}}{X(1+aV)}$.

2. Let X be an homogeneous function of one dimenſion of x, that is, $= ax$, and V a ſimilar function of n dimenſions of v, that is $= bv^n$, and X′ a ſimilar function of r dimenſions of x and v, and $n + r = 1$; then by ſubſtituting zx and its fluxion for v and its fluxion, can be found the fluent of the fluxional equation $(X + aVX')\dot{x} = -v\dot{v}$, and conſequently the velocity and time by the quadrature of curves in terms of the ſpace; and in like manner of many other caſes.

3. Fig. 4. Let a body moving in a given curve be acted on at any point P by a force f tending to a given point S, and reſiſted by a medium proportional to V a function of its velocity multiplied into its denſity X′ a function of the diſtance SP = D; to find its velocity, time, and diſtance from the given point S in terms of each other. Let $F = f \times \frac{PY}{SP}$ the force in the direction of the tangent PY, and conſequently $(F + VX')\dot{A} = -v\dot{v}$, and $v^2 = \frac{1}{2} C \times f$, where $\dot{A}$ is the increment of the arc, and C the chord of curvature in the direction SP; but ſince the curve is given, the chord of curvature may be deduced from the diſtance, &c. and the increment $\dot{A}$ of the arc from a function of the diſtance multiplied into the increment

of

of the diſtance; then, if f or v be a given function of the diſtance, the other may be deduced from it, and conſequently $-v\dot{v}=\varphi:(\mathrm{D})\times\dot{\mathrm{D}}$ will be a given function of the diſtance D multiplied into $\dot{\mathrm{D}}$, whence we have $\varphi:(\mathrm{D})\times\dot{\mathrm{D}}=\dot{\mathrm{D}}\,(f\times\frac{\mathrm{PY}}{\mathrm{SP}}+\mathrm{X'V})$ divide by $\dot{\mathrm{D}}$, and there reſults an algebraical equation, from which $\mathrm{V}\times\mathrm{X'}$ may be found.

If neither v nor f be given, reduce the two equations $(f\times\frac{\mathrm{PY}}{\mathrm{SP}}+\mathrm{VX'})\,\dot{\mathrm{A}}=-v\dot{v}$ and $v^2=\frac{1}{2}\mathrm{C}f$ into one, ſo as to exterminate either f or v and its fluxions, and there reſults an equation expreſſing the relation between the other v or f and D and their fluxions: from the velocity given in terms of D may be deduced the time from the equation $\dot{t}=\frac{\dot{\mathrm{A}}}{v}$.

3.2. If the body be acted on by forces tending to more points S, S′, S″, S‴, &c. in the ſame plane; reſolve each of the forces into two; one in the direction of the tangent, and the other perpendicular to it; let the ſum of the forces in the direction of the tangent be F; and in the direction perpendicular to it be F′; and 2R the diameter of curvature at the point P, which will be given in terms of the diſtances from two points, or of an abſciſs and ordinate, and their fluxions, &c.: aſſume the two equations before given $(\mathrm{F}+\mathrm{X'V})\,\dot{\mathrm{A}}=-v\dot{v}$ and $v^2=\mathrm{F'R}$, and ſince $\dot{\mathrm{A}}$ is always given in terms of D and $\dot{\mathrm{D}}$, if F and F′ be given in terms of D, D′, &c. the value of $\mathrm{V}\times\mathrm{X'}$ may be acquired by a ſimple algebraical equation: but if F and F′ be not given, and conſequently v not given, but V a given function of v, and X′ a given function of the above mentioned diſtances; then ſubſtitute for v its value $\surd(\mathrm{F'R})$ in the function V, and the fluxion of $\frac{1}{2}\mathrm{F'R}$ for $v\dot{v}$, and there will reſult an equation involving D and F′ and their fluxions, and F; but if the forces tending to all the points but one are given in terms of the diſtance D, or abſciſs or ordinate of the curve, and their fluxions; then from F′ can be found F, and, *vice verſâ*, from F can be found F′, and conſequently there reſults a fluxional equation expreſſing the relation between F or F′ and the diſtance D or D′, &c. or abſciſs or ordinate, and their fluxions.

From F and F′, and conſequently v being found in terms of D, D′, &c. can be deduced $\dot{t} = \frac{\dot{v}}{F}$.

The ſame method may be applied, if ſome forces tend to an infinite diſtance, that is, act parallel to themſelves, and others tend to given points.

Ex. Let the accelerating force be directly as the arc $= x$, and the reſiſtance uniform $= a$; then will $(x - a)\,\dot{x} = -v\dot{v}$, and conſequently $x^2 - 2ax + B = -v^2$; let A be the arc, where the velocity $= o$; then will the equation $A^2 - 2aA - x^2 + 2ax = v^2$, and the increment of the time $\dot{t} = \frac{\dot{x}}{v} = \frac{\dot{x}}{\sqrt{(A^2 - 2aA - x^2 + 2ax)}}$, whoſe integral is $\frac{1}{A-a} \times$ arc of a circle, of which the radius is $A - a$ and coſ. $= x - a$, where A is the diſtance of the point from which the body begins to fall, and the loweſt point of the curve; and the accelerating force $x - a$ is as the diſtance from a point (a) of a curve, of which the diſtance from the loweſt is a.

Cor. The times of the body falling from any point of the curve to a will be equal.

Cor. The body on this hypotheſis will either reſt at the point a, or at the loweſt point, or any point between $+a$ and

$-a$; for it may reſt at any point, where the reſiſting force is always equal or greater than the accelerating force.

Cor. Let n be the number of vibrations, then the diſtance of the arc, to which it will aſcend from the loweſt point at n vibrations, will be $A-2na$; if $A-2na$ be not greater than $2a$, it will never paſs the loweſt point.

Philoſophical enquiries require ſome corrections, which do not enter into mathematical calculus; for example, in ſome caſes the calculus changes the quantities from negative to affirmative, &c. when from philoſophical conſiderations they are not changed; and, *vice verſâ*, they may be changed to affirmative, &c. on philoſophical conſiderations, when they are not changed from the calculus: and alſo a body may ſtop, &c. from philoſophical conſiderations, as in the preceding example, when it does not follow from the algebraical calculus, &c. It is further to be obſerved, that reſiſtances are always to be taken affirmatively.

Ex. 2. Let the accelerating force be as the arc, that is, the diſtance from the loweſt point, and the reſiſtance as the velocity; then will the fluxional equation $(F-V)\dot{A}=-v\dot{v}$ be $(ax-v)\dot{x}=-v\dot{v}$, which is an homogeneous equation of the firſt order: write in it zx for v, and its fluxion for $\dot{v}$, and there reſults the equation $(ax-zx)\times\dot{x}=-zx^2\dot{z}-z^2x\dot{x}$, whence $(a-z)\dot{x}=-zx\dot{z}-z^2\dot{x}$ and $\frac{\dot{x}}{x}=\frac{-z\dot{z}}{a-z+z^2}$, and thence log. $x=-\frac{1}{2}$ log. $(a-z+z^2)$ (W) $-\frac{2}{4a-1}\times$ cir. arc, whoſe radius is $\frac{\sqrt{4a-1}}{2}$ and tangent $(z-\frac{1}{2})+B$, whence can be found $v=x\dot{z}$, and from curvilinear areas $\dot{t}=\frac{\dot{x}}{v}$.

If $4a$ is leſs than 1, then it becomes log. $x = W - \frac{1}{4\sqrt{\frac{1}{4}-a}} \times$ log. $\frac{z-\sqrt{\frac{1}{4}-a}-\frac{1}{2}}{z+\sqrt{\frac{1}{4}-a}-\frac{1}{2}} + B$; where B is an invariable quantity to be aſſumed according to the conditions of the problem.

Cor. If the force be directly as the diſtance, or as the arc of the curve from the body to the loweſt point, and the reſiſtance as the velocity; then will the velocity in one arc be to the velocity in the correſponding point of another arc, as the arcs to be deſcribed; and conſequently the times equal.

4. If the body is acted on by forces tending to points S, S′, S″, &c. ſituated in different planes, then let F be the ſum of the forces in the direction of the tangent at the point P; F′ and F″ the ſum of the forces acting on the body in two different directions at the ſame point, which are not ſituated in the ſame plane with the tangent and each other; from the three equations $(F + X'V)\dot{A} = -v\dot{v}$ and $\frac{v^2}{C} = \frac{1}{2}F'$ and $\frac{v^2}{C} = \frac{1}{2}.F''$, in which the ſame letters denote the ſame quantities as before, and C and C′ denotes the chords of curvature in the ſame directions as the forces F′ and F″, which from the curve being given can be found at any point; and if F′ or F″ is given in terms of the diſtance from a given point, or an abſciſs or ordinate, &c. the velocity v can be found in terms of the ſame, and X′V by a ſimple algebraical equation: if F′ is not given, and V is a given function of v, ſubſtitute in V for v its value $\sqrt{(\frac{1}{2}C \times F')}$, and there reſults an equation expreſſing the relation between F (which can be deduced from F′ or F″) and the diſtance of the body from ſome given point, or the abſciſſæ and ordinates of the curve required, and their fluxions.

If ſome of the forces act in parallel directions; the forces, velocities, &c. may be found by the ſame method.

PROP. X.

Fig. 11. Let a body be projected in a direction HL with a given velocity, and be acted on by a force in a direction parallel to AP $=x$, which varies as X a function of x; and alſo by another force in a direction parallel to MP $=y$, that is, perpendicular to AP, which force varies as Y a function of y; and let it move in a medium, of which the reſiſtance is proportional to the velocity; to find the curve deſcribed.

Find the fluent of $(X+av)\,\dot{x}=-v\dot{v}$, which corrected according to the conditions of the problem (*viz.* ſo that v at the point H may be to the velocity of projection :: Hc : Hb, where bc is drawn perpendicular to AP) ſuppoſe $v=X'$; find the fluent of $\frac{\dot{x}}{X'}$, which corrected ſo as to become $=o$, when $x=$ AH, let be X''. In the ſame manner find the fluent of $(Y+a'v')\,\dot{y}=-v'\dot{v}'$, which corrected, ſo that v' at the point H may be to the velocity of projection :: cb : Hb, ſuppoſe $v'=Y'$; find the fluent of $\frac{\dot{y}}{Y'}$, which corrected ſo as to become $=o$, when PM $=o$, let be $=Y''$; aſſume $X''=Y''$, and thence from x find y: take AP $=x$ and PM $=y$, and M will be a point in the curve, which a body projected in the line HL deſcribes; and if Mm in the direction parallel to HAP : mo perpendicular to it :: velocity v : velocity v', then will Mo be a tangent to the curve in the point M.

2. If a body is acted on by forces tending to any given points S, S′, S″, &c. which vary as given functions of their diſtances from the body, and reſiſted by a force which varies according to a given function V of the velocity (v) into its denſity X′,

where X' varies according to ſome function of the diſtances from the given points, &c. ; to find the curve deſcribed.

1. From the diſtances of the body from two given points, or the abſciſs and ordinate of the curve deſcribed, and their fluxions, &c. find the forces acting in the direction of the tangent to the curve, and in ſome other direction, which ſuppoſe F and F'; and alſo the chord of curvature in the above mentioned direction, which let be C; then from the equations $(F + V \times X') \dot{A} = -v\dot{v}$ and $v^2 = \frac{1}{2} C \times F$ reduced into one by writing for v its value in the function V, and for $v\dot{v}$ its value deduced from the equation $v^2 = \frac{1}{2} C \times F$, and for $\dot{A}$ (the fluxion of the arc) its value deduced from the diſtances, &c. will reſult an equation expreſſing the relation between the diſtances from two given points to the curve, or its abſciſs and ordinates, and their fluxions.

3. If the forces are not all ſituated in the ſame plane, then from the before given equation $(F + V \times X') \dot{A} = -v\dot{v}$, and the two others $v^2 = \frac{1}{2} C \times F'$ and $v^2 = \frac{1}{2} C'F''$, where F denotes the force in the direction of the tangent, and F' and F'' are the forces in different directions, which both are not ſituated in the ſame plane with each other and the tangent, and in which directions the chords of curvature are reſpectively C and C'; ſince the quantities F, F', and F''; C and C' and $\dot{A}$ (as proved before) can all be expreſſed in terms of the diſtances from three given points, or from two abſciſſæ and one ordinate, and their reſpective fluxions; may be deduced two fluxional equations expreſſing the relation between the diſtances from three given points, or two abſciſſæ and an ordinate, &c.

The ſame principles may be applied to caſes, in which ſome of the forces act in parallel directions.

On moveable Centers.

PROP. XI.

1. Given the reſpective places of (n) bodies S, S′, S″, S‴, &c. in the curves A, A′, A″, A‴, &c. at the ſame time, and in the ſame plane, and the forces of all the bodies acting on S, except two, S′ and S″; to find the forces of the two bodies S′ and S″ on the body S.

This propoſition may be reſolved by the method given in Prop. 4. for to produce the ſame effect the ſame finite forces will be requiſite, whether the centers of forces reſt or move in given curves.

1.2. If the bodies S, S′, S″, &c. move in different planes, then all the forces acting on the body, except three, may be given, which may be acquired from the method given in the ſame propoſition.

Hence it appears, that $2n$ forces may be requiſite to be found from the conditions of the problem to determine all the bodies to move in their reſpective curves, when they are all ſituated in the ſame plane, and that $3 \times n$ forces may be requiſite in different planes, &c. if the force of one body (S′) on another (S″) does not at all depend on the force of the ſame body (S′) on any other (S‴); and if the ſame can be prædicated of the reſt, then $n \,.\, \overline{n-3}$ forces of the above mentioned bodies in the ſame, or $n \,.\, \overline{n-4}$ forces in different planes may be aſſumed at will.

3.

3. If the velocities v, v', v'', &c. at every point of the arcs a, a', a'', &c. of the (n) above mentioned curves A, A′, A″, &c. be given in terms of their arcs, abſciſſæ, or ordinates, &c. and the places in which the bodies are ſituated at the ſame time in the arcs b, b', b'', &c. of ſome other curves B, B′, B″, &c. find the correſponding velocities V, V′, V″, &c. at the ſame time of the bodies in the curves B, B′, B″, &c.; then make $\frac{\dot{a}}{v} = \frac{\dot{a}'}{v'} = \frac{\dot{a}''}{v''} =$ &c. $= \frac{\dot{b}}{V}$, or which is equal to it $= \frac{\dot{b}'}{V'}$ or $= \frac{\dot{b}''}{V''} =$ &c. From the fluents of the fluxional equations reſulting properly corrected will be found the arcs a, a', a'', &c. deſcribed by the bodies in the curves A, A′, A″, &c. in the ſame time as the correſpondent arcs b, b', b'' &c.; and from thence, by the method given in the preceding caſe, may be deduced the forces.

The ſame principles may be applied to bodies moving in reſiſting mediums.

PROP. XII.

Given the law of the forces of two bodies acting on each other, to find the two curves by them deſcribed.

Fig. 12. Aſſume x and y for the abſciſs (AP) and ordinate (PM) of one curve, and z and u for the abſciſs (AP′) and ordinate (P′M′) of the other; where the abſciſſæ AP and AP′ begin from the ſame point A, and are ſituated in the ſame line; then will the diſtance (D = M′M) between the bodies = $\sqrt{\overline{z \pm x}^{2}} + \sqrt{\overline{u \pm y}^{2}}$); let the forces of the body placed at M on that at M′, and of the body placed at M′ on that at M vary as $\varphi : (D) = F$, and $\varphi' : (D) = F'$; and let $Mp = \dot{x}$ and $pm = \dot{y}$; then will coſine of the angle mMM′ to radius (1) be $\frac{y \pm u}{D} \times$

$\frac{\dot{y}}{\sqrt{(\dot{x}^2+\dot{y}^2)}} \pm \frac{x \pm z}{D} \times \frac{\dot{x}}{\sqrt{(\dot{x}^2+\dot{y}^2)}} = c$; and conſequently the force in the direction of the tangent M*m* will be $c \times F$, whence $-v\dot{v} = c \times F \times \sqrt{\dot{x}^2+\dot{y}^2}\ (\dot{A})$ and $v^2 = \frac{1}{2}CF$, where C is the chord of curvature in the direction of the force $(F) = \sqrt{1-c^2} \times 2\frac{(\dot{x}^2+\dot{y}^2)^{1\frac{1}{2}}}{\dot{y}\ddot{x}-\dot{x}\ddot{y}}$; and v the velocity of the body in the curve, whoſe abſciſs is x and ordinate y.

In the ſame manner let $\frac{x \pm z}{D} \times \frac{\dot{z}}{\sqrt{\dot{z}^2+\dot{u}^2}} \pm \frac{y \pm u}{D} \times \frac{\dot{u}}{\sqrt{\dot{z}^2+\dot{u}^2}} = c'$, the coſine of the angle made between the diſtance MM′ and arc of the curve of which the abſciſs is z and ordinate u, and conſequently $c' \times F'$ will be the force in the direction of its tangent, and therefore $-v'\dot{v}' = c' \times F' \times \sqrt{\dot{z}^2+\dot{u}^2}\ (\dot{A})$ and $v'^2 = \frac{1}{2}C'F'$, where C′ is the chord of curvature in the direction of the force $(F') = \sqrt{1-c'^2} \times 2\frac{(\dot{z}^2+\dot{u}^2)^{1\frac{1}{2}}}{(\dot{u}\ddot{z}-\dot{z}\ddot{u})}$, and v' the velocity of the body in the curve whoſe abſciſs is z and ordinate u; then, becauſe the times of deſcribing correſpondent arcs in the two curves are equal, their increments will be equal, and conſequently $\dot{t} = \frac{\sqrt{\dot{x}^2+\dot{y}^2}}{v} = \frac{\sqrt{\dot{z}^2+\dot{u}^2}}{v'}$; and there are deduced five fluxional equations, containing ſix variable quantities v, v'; x, y; z, and u, and their fluxions; reduce theſe equations, ſo that four of them (v, v', &c.) may be exterminated, and there will reſult an equation expreſſing the relation between x and y the abſciſs and ordinate of one curve, or z and u the abſciſs and ordinate of the other curve, and their fluxions; the fluential equation of which being found, and properly corrected, gives the equation to the curve.

The five equations are eaſily reduced to three by exterminating the quantities v and v'.

The fluxional equation reſulting will moſt commonly be of the fifth order, as evidently appears from the nature of the problem.

2. The ſame principles may be applied to determine the curves, when the bodies move in mediums, of which the reſiſtances are given: for example, ſuppoſe the reſiſtances to vary as a function of the diſtance from a given point into a function of the velocity: to the forces in the directions of the tangents contained in the preceding caſe muſt be added or ſubtracted the given reſiſtances for the forces in the directions of the tangents, and the remaining proceſs will be the ſame as is before given.

If two bodies deſcribe ſimilar orbits round a common center, either quieſcent or moving uniformly in a right line; the forces and velocities and reſiſtances of the medium will be to each other in correſpondent points as their reſpective diſtances from the center.

PROP. XIII.

Given the forces acting on any bodies, and tending to points either moveable or quieſcent, or in the direction of the tangents, &c.; to find the curve deſcribed by one of the bodies.

1. Aſſume x and y for the abſciſs and ordinate of the curve required, and from thence may be deduced the diſtances from any quieſcent center of force, and conſequently the force f in that direction; reſolve it into two others, one in the direction of the tangent, and the other in a different one; for example, let it be in a direction perpendicular to the tangent, and from their fluxions $\dot{x}$ and $\dot{y}$, and the force f may, by the method

before given, be deduced the forces in the two above mentioned directions; and in the ſame manner may be found from x, y, $\dot{x}$, and $\dot{y}$, the forces in the directions of the tangent and perpendicular to it, which follow from all the forces tending to given points, and acting on the body moving in the curve to be inveſtigated. 2. If ſome of the centers of force move in given curves B, B′, B″, &c. whoſe arcs let be denoted by B, B′, &c. and their reſpective places at the ſame time are given; then from their reſpective places given and forces, and x and y, and $\dot{x}$ and $\dot{y}$, can, as before, be deduced the forces in the direction of the tangent and its perpendicular to the curve required. 3. If other centers of forces move in given curves A, A′, A″, &c. and the velocities are given at every point of the curves; let A, A′, A″, &c. be the arcs of the curves A, A′, A″, &c. and ſuppoſe v, v', v'', &c. their correſpondent velocities; then, if the increments of the time be given, will $\frac{\dot{A}}{v} = \frac{\dot{A}'}{v'} = \frac{\dot{A}''}{v''} =$ &c. but as the velocities are given at every point of the curves, v in the curve (A) will be given in terms of its abſciſs, ordinate, arc, &c. and conſequently $\frac{\dot{A}}{v}$ in terms of the ſame quantities and their firſt fluxions; the ſame may be affirmed of the fluxions $\frac{\dot{A}'}{v'}$, $\frac{\dot{A}''}{v''}$, in the curves A′, A″, &c.; hence, from the equation $\frac{\dot{A}}{v} = \frac{\dot{A}'}{v'}$, can be deduced the relation between the abſciſs or ordinate, &c. of the curve A and its correſpondent abſciſs or ordinate, &c. of the curve A′; and ſo of the remaining curves; hence this caſe is reduced to the preceding; but it is neceſſary alſo, that the times of the bodies in the two caſes ſhould be the ſame, in order that the places may correſpond, and conſequently $\frac{\dot{A}}{v} = \frac{\dot{B}}{V}$, where V denotes the velocity of the body at any point of the curve B from which equation can be deduced the correſpondent abſciſſæ and ordinates, &c. of the curves B and A; and thence the two caſes are reduced to the preceding, whence the correſpondent forces in the directions of the tangent, and perpendicular to it, can be found as above. 4. If ſome (m) of the centers move in curves L, L′, L″, &c to be deduced from the laws of the forces being given which act on them; aſſume z and u, z' and u', z'' and u'', &c. for their reſpective abſciſſæ and correſpondent ordinates; and from them and y and x, $\dot{y}$ and $\dot{x}$, find the forces acting on the body moving in the curve required in the direction of the tangent, and perpendicular to it, as before; then add all the forces deduced which act perpendicular to the tangent and alſo all contained in the direction of the tangent together with the reſiſting force in the ſame direction, and let the ſums reſulting be reſpectively F and F′: by the ſame method find the ſum of the forces which act on the bodies moving in L, L′, L″, &c. in the directions of the tangents, and perpendiculars to them, which ſuppoſe S and s, S′ and s', S″ and s'', &c.; then reduce the 2 ($m+1$) equations of the formulæ found above, *viz.* $v^2 = \mathrm{F} \times \frac{\overline{\dot{x}^2+\dot{y}^2}^{1\frac{1}{2}}}{\dot{y}\ddot{x}-\dot{x}\ddot{y}}$ and $-v\dot{v} = \mathrm{F}' \sqrt{\dot{x}^2+\dot{y}^2}$; $v'^2 = s \times \frac{\overline{\dot{z}^2+\dot{u}^2}^{1\frac{1}{2}}}{\dot{u}\ddot{z}-\dot{z}\ddot{u}}$ and $-v'\dot{v}' = \mathrm{S} \times \sqrt{\dot{z}^2+\dot{u}^2}$; $v''^2 = s' \times \frac{\overline{\dot{z}'^2+\dot{u}^2}^{1\frac{1}{2}}}{\dot{u}'\ddot{z}'-\dot{z}'\ddot{u}'}$ and $-v'' \times \dot{v}'' = \mathrm{S}' \times \sqrt{\dot{z}''^2+\dot{u}''^2}$, &c.; where v, v', v'', v''', &c. reſpectively denote the correſpondent velocities of the bodies moving in the curves, whoſe abſciſſæ are x, z, z', z'', &c.; and alſo the ($m+1$) equations $\frac{\dot{\mathrm{B}}}{\mathrm{V}} = \frac{\sqrt{\dot{x}^2+\dot{y}^2}}{v} = \frac{\sqrt{\dot{z}^2+\dot{u}^2}}{v'} = \frac{\sqrt{(\dot{z}'^2+\dot{u}'^2)}}{v''} = \frac{\sqrt{(\dot{z}''^2+\dot{u}''^2)}}{v'''} =$ &c. containing the 3 ($m+1'$)

+ 1 variable quantities x and y, z and u, z' and u', z'' and u'', &c., v, v', v'', &c., and the variable quantity contained in B and V, into one, ſo that all the variable quantities except x and y and their fluxions may be exterminated, and there reſults an equation to the curve required expreſſing the relation between x and y its abſciſs and ordinate, and their fluxions.

5. If the forces are not ſituated in the ſame plane, aſſume X, x and y, for the two abſciſſæ and ordinates of the curve required; and Z, z and u; Z′, z' and u'; Z″, z'' and u''; &c. for the two abſciſſæ and ordinates of the (m) curves L, L′, L″, &c. reſpectively; and from the preceding method may be acquired the $3(m+1)$ equations $v^2 = F \times C$, $v^2 = F' \times C'$, and $-v\dot{v} = F'' \times \sqrt{\dot{X}^2 + \dot{x}^2 + \dot{y}^2}$; $v'^2 = S \times C'$, $v'^2 = \sigma c'$ and $-v'\dot{v}' = s \times \sqrt{Z^2 + \dot{z}^2 + \dot{u}^2}$; $v''^2 = S'C'' = \sigma' c''$ and $-v''\dot{v}'' = s' \times \sqrt{(\dot{Z}'^2 + \dot{z}'^2 + \dot{u}'^2)}$; $v'''^2 = S''C''' = \sigma'' c'''$ and $-v'''\dot{v}''' = s'' \times \sqrt{(\dot{Z}''^2 + \dot{z}''^2 + \dot{u}''^2)}$; &c.; in which v denotes the velocity in the required curve, and v', v'', v'''', &c. the correſpondent velocities in the curves L, L′, L″, &c.; and F, F′, and F″; S, σ and s; S′, σ' and s'; S″, σ'' and s''; &c. denote the forces acting on the reſpective bodies in two different planes and in the tangents, which planes cut each other in the tangents of the curves; and C and c, &c., C′ and c', &c., C″ and c'', &c. the $\frac{1}{2}$ chords or radii of curvature in thoſe two planes to the different curves in the directions of the forces; and alſo the ($m + 1$) equations beforementioned $\frac{\dot{B}}{V} = \frac{\sqrt{\dot{X}^2 + \dot{x}^2 + \dot{y}^2}}{v} = \frac{\sqrt{\dot{Z}^2 + \dot{z}^2 + \dot{u}^2}}{v'} = \frac{\sqrt{Z'^2 + \dot{z}'^2 + \dot{u}'^2}}{v''} =$ &c.; where $\sqrt{\dot{X}^2 + \dot{x}^2 + \dot{y}^2}$, $\sqrt{Z'^2 + \dot{z}'^2 + \dot{u}'^2}$, &c. are the fluxions of the arcs of the required curve, and of the curves L, L′, L″, &c. reduce theſe $4m + 4$ equations con-

taining $4m + 5$ variable quantities into two, ſo that all the variable quantities except three, X, x, and y, and their fluxions may be exterminated; and there reſult the two equations required.

It may be obſerved, that when the reſiſtance ariſing from the denſity of the medium and velocity (v) of the body varies as $X' \times v^{`} + X$, where X and X′ are as functions of the diſtances from given points, the reſolution of the fluxional equations will generally be more eaſy, than when the reſiſtance varies as other functions of the velocities.

If the force acts equally on the particles of the body and fluid, then the force by which a body deſcends in a medium is as the whole force X acting on the body at the given diſtance multiplied into a fraction whoſe numerator is the difference between the denſity of the body (D) and fluid (X′) at that diſtance and denominator D, that is, as $X \times \frac{D - X'}{D}$.

Many caſes might have been given, in which the fluxional equations could have been reſolved; but in general their fluents can only be found by means of converging ſerieſes.

Fig. 1.

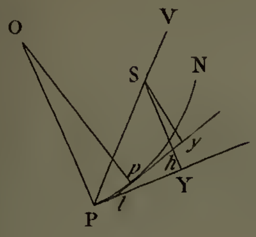


Fig. 2.

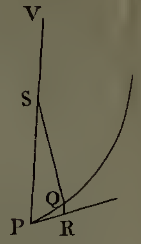


Fig. 3.

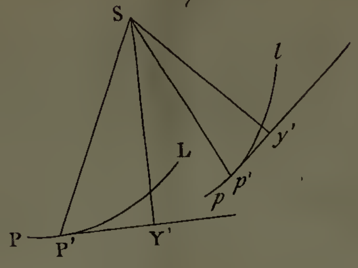
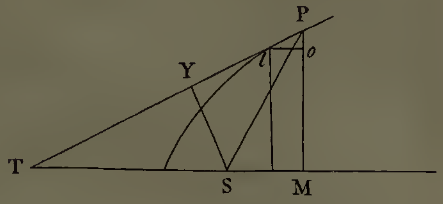


Fig. 4.



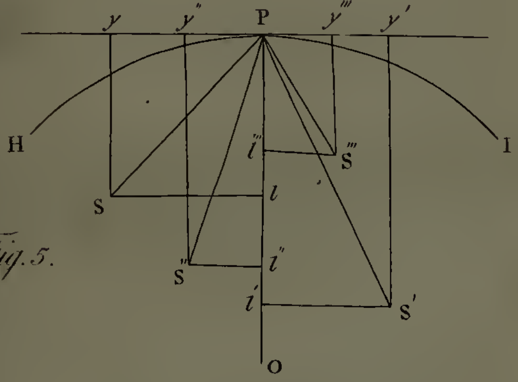


Fig. 5.

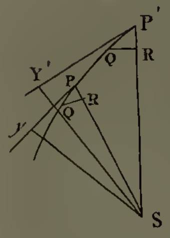


Fig. 6.

Fig. 7.

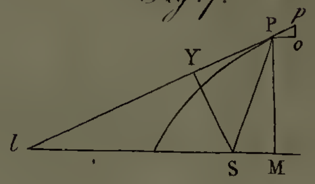


Fig. 8.

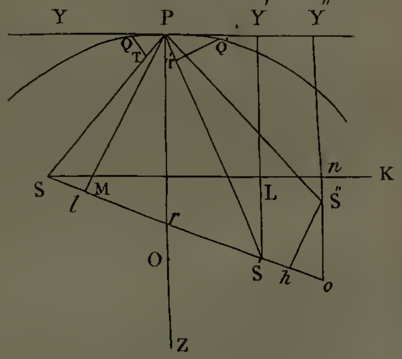
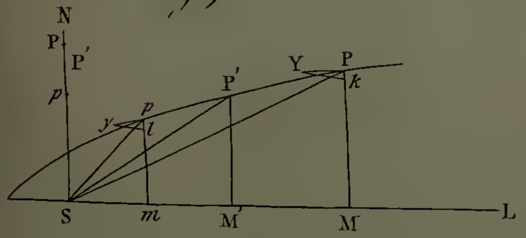


Fig. 9.



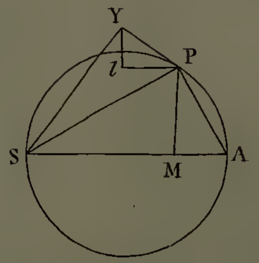
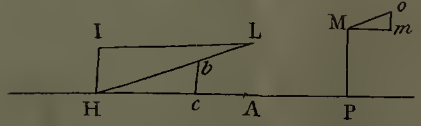


Fig. 10.

Fig. 11.



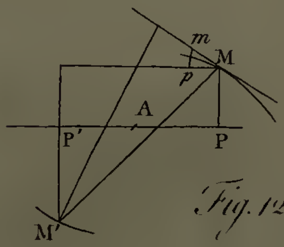


Fig. 12.

VIII. *Experiments on local Heat. In a Letter from* James Six, *Eſq. to the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read January 10, 1788.

DEAR SIR,

THE following experiments are a continuation of thoſe I had the pleaſure of communicating to you ſome time ſince, relating to the diverſity of local heat in the atmoſphere; and confirm, in a more particular manner, my former obſervations reſpecting a remarkable refrigeration, which, in clear weather, takes place near the earth; for, although its ſurface in the day-time is then moſt liable to be heated by the ſun, yet after that is ſet, and during the night, the air is always found coldeſt near the ground, particularly in vallies. This phænomenon, for want of nocturnal obſervations, has been but lately noticed*; probably becauſe in the day-time, eſpecially when the weather is ſtill and warm, the difference of temperature is reverſed, the reflection of the ſun's rays from the earth heating the air more below than it does above.

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* Within theſe few years.

Mr. Wilson, in the Philoſophical Tranſactions, 1780, p 467. and 1781, p. 368. deſcribing the effects of a ſevere froſt at Glaſgow, mentions a remarkable refrigeration, which he perceived to take place on the ſurface of the ſnow and hoar-froſt, which ſunk the mercury in a thermometer laid upon it many degrees lower than one ſuſpended twenty four feet above it. This phænomenon, he ſays, *had recently come under obſervation*; but ſuppoſing (I preſume) the principle of refrigeration to operate only on the ſurface of the ſnow, or hoar-froſt, when the cold was ſevere, it does not appear he made experiments at any other time.

The experiments in my former Paper * were made partly in autumn, and partly in winter; and, the local variations differing in ſome meaſure with the ſeaſons, I was deſirous of continuing a ſeries of experiments throughout one entire year. To this end, therefore, I ſuſpended proper thermometers † in a ſhady northern aſpect, in the open air, at different heights; one in my garden at nine feet, and another in the Cathedral Tower 220 feet from the ground; continuing my journal, with the omiſſion of a few days only, from July 1784 till July 1785.

In the ſummer of 1783, making experiments with thermometers, which I had conſtructed to ſhew the greateſt degrees of heat and cold that happened in the obſerver's abſence, *I firſt perceived* this nocturnal refrigeration in the loweſt ſtratum of the atmoſphere; for by ſuſpending theſe thermometers at different altitudes, and viewing them only once in twenty-four hours, I found the true maximum and minimum of the heat and cold, which had happened during that time in their ſeveral ſtations.

A writer in the Gentleman's Magazine, for March 1785, p. 170, who did me the honour to mention my former Paper, containing *Experiments on Local Heat*, obſerved, that the effects of a ſharp froſt, which happened in *December of the preceding year*, appeared to be much more ſevere among the vegetables in the vallies than on the hills; and by taking the difference of temperature with thermometers at that time, he found them vary from 5 to 17 degrees, according to their ſeveral ſtations.

M. DE SAUSSURE, in his Voyages dans les Alpes, Vol. II. publiſhed 1786, ſays, " Quant à l'obſervation bien curieuſe *et bien nouvelle* de M. PICTET, que pendant la nuit la couche d'air la plus baſſe depuis la terre juſques a 5 pieds au-deſſus eſt plus froide que les couches ſuivantes depuis 5 pieds juſque a 50."

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* Philoſophical Tranſactions, Vol. LXXIV. p. 428.

† Conſtructed to give the true maximum and minimum that may happen in the obſerver's abſence. See Philoſophical Tranſactions, Vol. LXXII. p. 72.

The reſult entirely correſponded with what I had before obſerved reſpecting the nocturnal diminution of heat, and the particular ſtate of the atmoſphere requiſite to produce it. The greateſt variations which happened within the year were in October and June; in the former month, the thermometers generally differed moſt in the night; in the latter, moſt in the day. From the 25th to the 28th of October, the heat below, in the night, exceeded, in a ſmall degree, the heat above, at which time there was frequent rain, ſometimes mingled with hail. From the 11th to the 14th, and alſo on the 31ſt, there was no variation at all; during which time likewiſe the weather was rainy; all the reſt of the month proving clear, the air was found colder below than it was above, ſometimes 9 or 10 degrees. In the month of June, the greateſt nocturnal variations happened from the 11th to the 15th, and from the 25th to the 30th; at both which times there appeared to be two currents of wind, the upper current from the S.W., the lower from the N.E.[*]. On cloudy nights, the loweſt thermometer ſometimes ſhewed the heat to be a degree or two warmer than the upper one; but in the day time the heat below conſtantly exceeded the heat above more than in the month of October. See Tab. I. and II.[†].

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[*] Sometimes theſe were rendered viſible by clouds in different ſtrata, moving in different directions, and at other times by clouds moving in a contrary direction to a very ſenſible current of air below.

[†] The obſervations of the wind and weather were taken at Canterbury. In all the tables, the nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.—With my thermometer I have ſometimes ſuſpended hygrometers; but their movement, although as conformable as poſſible to the moiſture of the air, had not a ſufficiently regular correſpondence with the variation of the thermometers to be inſerted in the tables

Being deſirous of knowing whether the nocturnal refrigeration increaſed on a nearer approach to the ſurface of the earth, I placed, in the midſt of an open meadow, on the bank of the river, near this city, two thermometers; one on the ground, and the other ſix feet above it; with theſe, and the two others before mentioned, one on the tower, and the other in my garden, I made obſervations from the 10th to the 23d of October, 1786. See Tab. III. But as the thermometer ſix feet from the ground, in the meadow, nearly agreed with that in the garden at nine feet, I have omitted an account of the latter in this table of compariſon. Here I found, as before, the nocturnal variations entirely regulated by the clearneſs, or the cloudineſs, of the ſky; and although they did not always happen in the ſame proportion to the reſpective altitudes, yet, when the thermometers differed at all, that on the ground was always the coldeſt.

Finding ſo conſiderable a difference as three degrees and a half, within ſix feet of the earth's ſurface, I increaſed the number of my thermometers in the meadow to four; one of them I ſunk in the ground, another I placed juſt upon the ground, a third I ſuſpended at three feet, and a fourth at ſix feet from the ground; at the ſame time I placed three thermometers in an open garden on St. Thomas's Hill, where the land is level with the Cathedral Tower, and about a mile diſtant from it; here I likewiſe put one in the ground, another juſt upon it, and ſuſpended a third ſix feet above it.

tables of compariſon. It is true, when the weather was clear and not too cold, the upper ones indicated dryneſs more than the lower; but in froſty weather, the contraction and expanſion of the hygrometers being impeded by the froſt, they did not in any regular manner agree with the variations of the thermometer.

With theſe ſeven thermometers and the two, before mentioned, in the city, I continued a diary for twenty days, taking alſo every morning the temperature of the water in the river; but the weather proving cloudy ſoon after, the thermometers hardly varied at all, ſeven or eight days only excepted. After this time I never rectified them but when the appearance of the weather gave me reaſon to expect that they would vary conſiderably. See the obſervations that were then taken Tab. IV.; by which it appears, that the cold in the night was generally greater in the valley than that on the hill; but that the variations between the thermometers on the ground, and thoſe ſix feet above them, were often as great on the hill as in the valley.

Perceiving now, that a difference of temperature was frequently found within three feet from the ground, I reſolved to try ſtill nearer; but my thermometers being eighteen inches in height could not be applied ſo near to the ſurface of the earth as my experiment required; I therefore conſtructed two others for the particular purpoſe; and by bending down the large tube, the bulb or body of the thermometer, to an horizontal poſition, while the ſmaller tubes remained in a vertical one, I was enabled to aſcertain the different degrees of heat from the ground to a ſingle inch.

At certain times, when the weather was clear, I expoſed theſe two horizontal thermometers in an open place, ſuſpending one of them, ſo as to bring the body of it within an inch of the ground, and the other nine inches above it. When the variation among the other thermometers was conſiderable, I found alſo a difference between theſe; the lower one indicating ſometimes more than two degrees leſs heat than the upper one, although they were placed ſo near to each other. See Tab. V.

From the foregoing experiments it appears, that a greater diminution of heat frequently takes place near the earth in the night-time, than at any elevation in the atmoſphere within the limits of my inquiry; and that the greateſt degrees of cold are at ſuch times always found neareſt to the ſurface of the earth; that this is a conſtant and regular operation of nature, under certain circumſtances and diſpoſitions of the atmoſphere, and takes place at all ſeaſons of the year; that this difference never happens in any conſiderable degree but when the air is ſtill, and the ſky perfectly unclouded*; but the moiſteſt vapour, ſuch as dews and fogs †, did not, as far as I could perceive, at all impede, but rather increaſe the refrigeration. In very ſevere froſts, when the air frequently depoſits a great quantity of frozen vapour, I have generally found it greateſt ‡; but the exceſs of heat, which in day-time, in the ſummer ſeaſon, was found at the lower ſtation, in the winter diminiſhed almoſt to nothing.

The foregoing experiments related to the difference of heat, which, at certain times, is found at different altitudes; the following to the different degrees of heat obſerved at different ſituations in reſpect to the ſea-ſhore. Tab. VI. and VII. contain a ſet of correſponding obſervations; among which are thoſe you were ſo obliging as to take for me at Chiſlehurſt*; others at the ſame time were taken in my garden, and on the Cathedral Tower; and others on the ſea-ſhore, about ſeven miles N.N.W. from Canterbury, where the thermometer was ſuſpended about 40 feet above high-water mark, 14 from the ground, and about 100 yards from the ſea†.

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* Free from clouds which float higher in the air than fogs, and are ſufficiently opaque to hide the ſtars from our view even in the zenith.

† When fogs covered the valley, the refrigeration below was conſiderable; but they very ſeldom reached ſo high as the upper thermometer, and the ſky was clear above. No variation during rain, except mingled with hail.

‡ This remarkable diminution of heat near the ſurface of the earth in clear weather, after the ſun is ſet, and in the night, proceeds, I apprehend, partly from the coolneſs which the dews or vapours poſſibly may acquire in their deſcent, and partly from the evaporation which takes place at the ſame time from all moiſt or frozen bodies expoſed to the open air, particularly from thoſe on which the dew or hoar froſt is depoſited.

By Tab. VI. it appears, that every night, one only excepted, during that time, the air was coldeſt at Chiſlehurſt; and that the mean heat at the ſea-ſhore was equal to that on the tower at Canterbury. In the month of June (Tab. VII.) it is remarkable, that the cold was ſtill greater in the night at Chiſlehurſt than at any of the other places, excepting where there appeared two currents of wind, the upper current from the S.W. and the lower from the N.E.; at which time alſo there was the greateſt difference between the thermometer in the garden and that on the tower.

Tab. VIII. contains obſervations taken only at particular times, when the weather was extremely cold; and ſhews how nearly at ſuch times the temperature in the night, at the ſea-ſhore, generally agreed with that on the Cathedral Tower; with this exception, however, that on the 18th, 21ſt, and 22d of February, 1784, a little ſnow falling at ſeveral times in the day, the evenings after being clear, and the wind at N.E., the thermometer on the ſea-ſhore, contrary to what uſually happened, gave the cold greater than the thermometer on the tower; but under ſimilar circumſtances I have ſince ſeen the ſame.

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* The thermometer at Chiſlehurſt was ſuſpended twelve feet from the ground.

† The thermometer on the Cathedral Tower was elevated about 200 above that on the ſea-ſhore.

The following experiments relate to the variation of local heat in the earth itſelf; the diverſity of which appears from the different heat of the water iſſuing from it at different places*. It has been conjectured, that the diverſity of the temperature of ſprings may probably depend on their different elevations in the earth, with reſpect to the level of the ſea.

Two remarkably deep wells, both near the ſea-ſhore, and not far diſtant from Canterbury, gave me a favourable opportunity of making experimental inquiry into this matter; eſpecially as the ſituation of the two ſprings differed conſiderably from each other in reſpect to the level of the ſea. One of theſe is a well in Dover Caſtle, which is ſunk 360 feet through the high cliff of chalk on which the Caſtle ſtands, and the depth of the well is nearly equal to the height of the cliff from the ſea. The other is King's-Well at Sheerneſs †, which was ſunk 330 feet through almoſt one entire ſtratum of firm clay, where the ſurface of the ground is only four feet above high water.

Suppoſing therefore the ſpring in Dover well to lie level with the ſea, the ſpring of the well at Sheerneſs lies 326 feet below it; a circumſtance extremely favourable to my experiment.

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* The temperature here meant is not that of the very hot ſprings which proceed from particular local cauſes, ſometimes riſing near the ſurface of the earth; nor of ſprings which are near enough to the ſurface of the earth to be affected by the change of temperature in the atmoſphere; but that heat which being found at a great depth is more likely to be permanent, and may be ſuppoſed to originate from ſome general cauſe in the earth.

† See an account of ſinking this curious well by Sir THOMAS PAGE, Phil. Tranſ. Vol. LXXIV. p. 6.

ment. The temperature of the ſprings I took in the following manner.

After fathoming each well with a line and plummet, I let one thermometer down to the bottom, and fixed another on the line, ſo as to reach to half the depth only, keeping a third to take the temperature of the air at the top.

Degrees of heat in the wells.

Sept. 28, 1784. Temperature of the water in the new well in Dover Caſtle.

By thermometer at the top	56°
By ditto at the middle	52
By ditto at the bottom	48¾

Found the well 360 feet deep with 21 feet water.

Oct. 6, 1784. Temperature of the water in King's Well at Sheerneſs.

By thermometer at the top	53°
By ditto at the middle	51
By ditto at the bottom	56

Found the well 280 feet deep[*] with 180 feet water.

About noon was the time of day when I made the experiments at both places, and the top of the reſpective wells varying from each other depended wholly on the accidental temperature of the atmoſphere at the time; but that the thermometer at half the depth of the well at Dover gave nearly the mean heat of the top and bottom, while that in a correſponding ſituation in the well at Sheerneſs gave it colder than either top or bottom, I attribute to the following circumſtance.

Over the well at Sheerneſs a machine is erected, which raiſes the water by means of an horizontal windmill, working an endleſs chain. This chain, conſiſting of jointed double bars, with a number of buckets fixed thereon, at certain diſtances from each other, continually deſcending into, and aſcending out of the water, to an elevation of eight or nine feet above the top of the well, may be ſuppoſed to reduce the water as far as it reaches to the mean temperature of the air above; and thus I found it; for 51 degrees had been the mean temperature of the air near the ſea-ſhore for ſeveral days before. At the bottom of the well, near to which the chain never deſcends, I found the temperature 56 degrees; above 7 degrees warmer than that at Dover well.

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[*] The ſand brought up from the bottom of the well by the force of the ſpring has reduced it to its preſent depth.

The water at the bottom of theſe wells is, I preſume, too deep beneath the ſurface of the earth ever to be affected by the temperature of the atmoſphere; for if the heat of the ſummer could have had any influence on either of them, that at Dover muſt have been moſt conſiderably affected by it, eſpecially in the month of September; and the air was ſomething warmer when the experiment was made at Dover than at Sheerneſs. From the nature of the different kinds of ſtrata in which theſe wells are dug, had they been in all other circumſtances the ſame, one might reaſonably expect to find the warmer ſpring in the chalk, and the colder in the clay; but here the reverſe is ſeen, without any apparent local cauſe, except the different elevations of the ſprings in reſpect to the level of the ſea.

I am, &c.

JAMES SIX.

Canterbury,
Auguſt 1, 1787.

TABLE.

TABLE I.

Thermometers at different ſtations compared together.

1784	Night. In the garden.	Night. On the tower.	Night. Difference.	Day. In the garden.	Day. On the tower.	Day. Difference.	Morning.	Afternoon.	Evening.	Wind.	Barometer.
Oct.	°	°	°	°	°						In.
1	40½	45	− 4½	56	53	+ 3	Partly clear.	– –	– –	NE	30.2
2	42	46	− 4	58	56	+ 2	Cloudy.	– –	Clear.	E	30.3
3	40	45½	− 5½	55½	54	+ 1½	Partly clear.	– –	Cloudy.	E	30.4
4	40	46	− 6	55½	54	+ 1½	Partly clear.	– –	– –	E	30.2
5	41	51	− 10	57½	56	+ 1½	Partly clear.	– –	Clear.	E	29.9
6	41	47	− 6	56	55	+ 1	Clear.	– –	– –	E	30.1
7	40	46	− 6	54½	52½	+ 2	Clear.	– –	– –	E	30.1
8	40	46½	− 6½	55	53	+ 2	Clear.	– –	– –	E	29.9
9	39½	43	− 3½	53	51	+ 2	Cloudy.	– –	Partly clear.	E	29.9
10	44½	45	− 0½	50½	49	+ 1½	Cloudy.	– –	Rain.	E	29.9
11	44	44	——	54	51	+ 3	Showery.	– –	– –	NW	30.0
12	44	44	——	55½	53	+ 2½	Showery.	– –	Cloudy.	NE	30.2
13	45	46	− 1	56	55	+ 1	Showery.	– –	– –	NE	30.2
14	44	44	——	50	48½	+ 1½	Cloudy.	Clear.	– –	E	30.1
15	31½	41	− 9½	50½	49	+ 1½	Clear.	– –	– –	NE	30.0
16	28½	38½	− 10	53	51½	+ 1½	Clear.	– –	– –	E	30.0
17	28	38½	− 10½	54	52½	+ 1½	Partly clear.	– –	– –	E	30.0
18	29	38½	− 9½	56	53	+ 3	Clear.	– –	– –	E	29.9
19	34	40	− 6	58	54	+ 4	Partly clear.	– –	Clear.	NE	29.7
20	30½	37	− 6½	54½	51½	+ 3	Clear.	Cloudy.	Clear.	NE	29.9
21	33	35	− 2	51	52	− 1	Clear.	– –	– –	NE	30.1
22	43½	44	− 0½	55½	54	+ 1½	Cloudy.	– –	Partly clear.	SW	30.0
23	43	46	− 3	56	54	+ 2	Miſty rain.	– –	Cloudy.	W	29.7
24	32½	34	− 1½	45½	43	+ 2½	Clear.	– –	Rain.	NW	29.4
25	31	30½	+ 0½	43	40½	+ 2½	Cloudy.	– –	– –	NW	29.7
26	35½	32½	+ 3	44	43½	+ 0½	Cloudy.	Rain with hail.	– –	NE	29.9
27	40	39	+ 1	47	46	+ 1	Rain.	– –	– –	NE	30.0
28	42	41	+ 1	47½	46	+ 1½	Rain.	– –	– –	NE	30.0
29	40	42	− 2	47	48	− 1	Cloudy.	– –	Rain.	NE	30.0
30	40	40	——	51	50	+ 1	Cloudy.	– –	Rain.	N	29.9
31	45	44	+ 1	50	49	+ 1	Cloudy.	– –	– –	W	29.8

TABLE II.

Thermometers at different ſtations compared together.

1785	Night.			Day.			Morning.	Afternoon.	Evening.	Wind.	Barometer.
	In the garden.	On the tower.	Difference.	In the garden.	On the tower.	Difference.					
June	°	°	°	°	°	°					In.
1	47	46	+ 1	65	59	+ 6	Showery.	– –	– –	W	29.8
2	47	48	− 1	65	61	+ 4	Clear.	Cloudy.	– –	N	29.8
3	46	47	− 1	71	67	+ 4	Partly clear.	– –	Clear.	SW	29.9
4	42½	46	− 3½	70	66	+ 4	Partly clear.	– –	Clear.	SW.NE	29.8
5	54	54	—	68	60	+ 8	Partly clear.	Cloudy.	Rain.	W	29.7
6	51	49	+ 2	61	57	+ 4	Cloudy.	– –	– –	S	30.0
7	43	45	− 2	62	58	+ 4	Cloudy.	– –	Rain.	SE	30.0
8	53½	52½	+ 1	73	66	+ 7	Rain.	– –	Showery.	SSW	30.0
9	56	55	+ 1	75	70	+ 5	Partly clear.	– –	– –	SW	30.2
10	56	55	+ 1	78	74	+ 4	Clear.	– –	– –	W	30.3
11	54	57½	− 3½	84	74	+ 10	Clear.	– –	– –	SW	30.4
12	53	57	− 4	71	63	+ 8	Clear.	– –	– –	SW.NE	30.3
13	41	49	− 8	67½	64	+ 3½	Clear.	– –	– –	SW.NE	30.2
14	42½	50½	− 8	69	64	+ 5	Clear.	– –	– –	SW.NE	30.2
15	48	53	− 5	78	73	+ 5	Clear.	– –	Hazy.	SW.NE	30.0
16	54	54	—	68	60	+ 8	Clear.	– –	– –	W	29.9
17	48	46	+ 2	61	60	+ 1	Showery.	– –	Clear.	N	29.8
18	50	49	+ 1	64	61	+ 3	Cloudy.	Partly clear.	– –	N	30.2
19	48	48	—	61	57	+ 4	Cloudy.	– –	Clear.	NE	30.3
20	45	48	− 3	65	60	+ 5	Cloudy.	Clear.	Cloudy.	NE	30.3
21	51	49	+ 2	64	59	+ 5	Cloudy.	– –	– –	NE	30.3
22	47	45	+ 2	64½	60½	+ 4	Cloudy.	Clear.	– –	NEN	30.3
23	51	49½	+ 1½	61	57	+ 4	Miſty rain.	– –	Cloudy.	NE	30.3
24	52	51	+ 1	62	58	+ 4	Cloudy.	Clear.	– –	NE	30.4
25	40	48	− 8	63	60	+ 3	Cloudy.	– –	Clear.	SW.NE	30.3
26	44½	49	− 4½	68	63½	+ 4½	Cloudy.	– –	Clear	SW.NE	30.2
27	43	52	− 9	67	61	+ 6	Clear.	Partly clear.	Cloudy.	SW.NE	30.0
28	56	55	+ 1	70	66½	+ 3½	Partly clear.	– –	Clear.	SW.NE	29.9
29	49½	55	− 5½	72	68	+ 4	Clear.	– –	– –	SW.NE	29.9
30	50	55	− 5	75	69	+ 6	Clear.	– –	– –	SW.NE	29.8

TABLE III.

Thermometers at different ſtations compared together.

1786	Night.					Day.					Morning.	After-noon.	Even-ing.	Wind.	Barometer.
	In the meadow on the ground.	In the meadow at ſix feet.	On the tower.	Diff. between the ground and ſix feet	Diff. between the ground and the tower.	In the meadow on the ground.	In the meadow at ſix feet.	On the tower.	Diff. between the ground and ſix feet.	Diff. between the ground and the tower.					
Oct		°	°	°	°	°	°	°	°	°					In.
10	52	52	52½	—	— 0½	62	62	58	—	+ 4	Cloudy.	– –	– –	S	29.5
11	38	40	48	— 2	— 10	57	58	57	— 1	—	Partly clear.	– –	Cloudy.	S	29.5
12	51	51	51½	—	— 0½	58	57	54	+ 1	+ 4	Cloudy.	– –	Rain.	SW	29.5
13	36	37	38½	— 1	— 2½	52	51	47	+ 1	+ 5	Miſty rain.	Rain.	Clear.	NW	30 0
14	25½	28	32	— 2½	— 6½	51	51	48	—	+ 3	Clear.	– –	– –	NNW	30.1
15	31	33	40	— 2	— 9	52	50	42	+ 2	+ 10	Clear.	– –	– –	NNW	30.1
16	29	31½	36	— 2½	— 7	54	53	48	+ 1	+ 6	Clear.	– –	– –	NNW	30.1
17	33	36½	39	— 3½	— 6	50	50	50	—	—	Partly clear.	– –	Clear.	N	30.2
18	31	34	34	— 3	— 3	53	53	50	—	+ 3	Clear.	– –	– –	N	30.2
19	26	29	36	— 3	— 10	50	49	47	+ 1	+ 3	Clear.	Cloudy.	Clear.	N	30.2
20	43	44	45	— 1	— 2	48	47	47	+ 1	+ 1	Cloudy.	– –	– –	W	30.3
21	43	44	45	— 1	— 2	49	49	48	—	+ 1	Cloudy.	– –	– –	W	30.3
22	45	45	45	—	—	52	52	47	—	+ 5	Cloudy.	– –	– –	E	30.2
23	45	45	45	—	—	51	51	51	—	—	Cloudy.	Miſty rain.	– –	E	30.2

TABLE IV.

Thermometers at different ſtations compared together.

1786	Night.							Day.							Greateſt difference in the night.	Wind.	Weather.	Barometer.	In the ground in the meadow.	In the ground on the hill.	Temperature of the river.
	In the meadow on the ground.	In the meadow at three feet.	In the meadow at ſix feet.	On the hill on the ground.	On the hill at ſix feet.	In the garden at nine feet.	On the tower at 220 feet.	In the meadow on the ground.	In the meadow at three feet.	In the meadow at ſix feet.	On the hill on the ground.	On the hill at ſix feet.	In the garden at nine feet.	On the tower at 220 feet.							
	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°			In.	°	°	°
Nov. 12	23	25½	26	25½	28½	27	31	43½	43	43½	43½	42	43	41½	8	E	Clear.	29.3	42	41	42
13	21½	24	25	26	29	26	30	37	36½	37½	36	38	36	35	8½	E	Clear.	29.7	42	41½	40
14	23	24½	25	22½	23½	24	26	34	33	33½	35	38	33	31½	3½	E	Clear.	29.8	42	41	37½
15	29	29	29	28½	28½	29	28½	37	37½	38	38	38	38	38	0½	E	Cloudy.	29.7	41½	39½	38½
27	35	37	38	39½	42	39	44	48	47½	48	47	47	47	46	9	S	Clear.	30.0	42	41	43
28	44½	45	45	44½	45	45	45	51	51	51	50	49	51	50	0½	S	Rain.	29.7	47	45	47
29	37½	38½	38	38	40	38	43	51	51	51½	50	48	51	51	5½	S	Rain.	29.7	44	43	44
30	39½	40	40½	39	40	39	40	49	50	50½	49	45	43	40	1½	S	Miſtyrain	29.7	44	43	45
Dec. 27	4	4	7	11	15	6½	21	43	43	42	42	42	42	44	17	S	Clear.	29.5			
29	25	26	28	29	32	27	34	46	47	51	46	47	45	46	9	S	Clear.	29.7			
31	28	29	31½	30½	31	29	35	39	40½	43	41	42½	43	37	7	WSW	Clear.	29.5			
1787																					
Jan. 2	25	27	29	26	28	28½	37	38	38	38	37	36	38	37	12	E	Clear.	30.4			
4	34	35½	38½	35	36	38	37½	44	45	46	44	44	44	44	4½	SW	Clear.	30.5			
7	24½	25½	28	27	27	26½	30½	35½	34	37	44	45	43	44	6	E	Clear.	30.5			
1	21	23	26	28	30	24½	31	41	42	44	43	43	37	38	10	E	Clear.	29.8			
28	13	14	17	12	16	16	25	33	34	37½	—	—	36	—	12	—	Clear.	29.8			
Feb. 6	28	29	32	31	33	31	36	45	45	48	44	44	43	43	8	S	Clear.	29.9			

TABLE V.

Thermometers at different ſtations compared together.

	Greateſt cold in the night.							
	Horizontal thermometers.			Vertical thermometers.				
1786	One inch above the ground.	9 inches above the ground.	Difference.	Meadow on the ground.	Meadow three feet above the ground.	Meadow ſix feet above the ground.	Difference on the ground and three feet above.	Diff. on the ground and ſix feet above.
Dec. 31	27½°	29°	1½°	28°	29°	31°	1°	3°
1787								
Jan. 3.	33½	34	0½	33	33	34	—	1
4	37½	38	0½	34	35	38	1	4
8	21	22	1	21	22	24	1	3
11	24½	27	2½	27½	28	31	0½	3½
12	21	23	2	21	23½	26	2½	5
13	30½	32	1½	34	34½	37	0½	3
14	22½	24	1½	23	25	28	2	5
Feb. 6	27½	29½	2	28	29	32	1	4
23	18½	20	1½	19	21	23	2	4

TABLE VI.

Thermometers at different ſtations compared together.

1784	Night.				Day.								
	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.	Morning.	Afternoon.	Evening.	Wind.	Barometer.
Oct.	°	°	°	°	°	°	°	°					In.
22	38	43½	44	47	52	55½	54	55	Cloudy.	– –	– –	SW	30.0
23	44	43	46	46	52	50	54	50	Miſty rain.	Partly clear.	Cloudy.	W	29.7
24	31	32½	34	34	42	46½	43	41	Clear.	– –	Rain.	NW	29.4
25	24	31	30½	33	36	43	40	40	Partly clear. Hail	Clear.	Cloudy.	NW	29.7
26	29	35½	32½	36	45	44	43	45	Partly clear. Hail.	– –	Showery.	NE	29.9
27	31	40	39	40	44	47	46	46	Rain.	– –	Cloudy.	NE	30.0
28	33	42	41	42	46	47½	46	47	Rain.	– –	– –	NNE	30 0
29	33	40	42	43	46	47	48	47	Cloudy.	Rain.	Showery.	NNE	30.0
30	33	42	40	41	49	49	50	50	Cloudy.	– –	Rain.	N	29 9
31	42	45	44	44	45	50	49	47	Cloudy.	Clear.	Cloudy.	NW	29.8
Nov.													
1	40	41	42	42	49	55	51	50	Rain.	Showery.	– –	SW	29.7
2	39	42	44	44	46	43½	49	47	Clear.	– –	– –	W	29.8
3	—	32½	35½	34	49	50	48½	51	Cloudy.	Clear.	– –	NW	29.9
4	34	34	38	37	48	48½	48	51	Cloudy.	– –	– –	S	29.8
5	32	33	38	38	43	45½	45	45	Miſty rain.	– –	– –	E	29.6
6	39	42	42½	44	44	46	43	45	Miſty rain.	– –	– –	E	29.5
7	38	42	42	42	44	46	44	46	Miſty rain.	– –	– –	E	29.8
8	27	35	36	41	39	45½	42	45	Clear.	– –	– –	S	30.0
9	27	28½	32	30	44	46	45	46	Cloudy.	Rain.	– –	S	29.7
10	37	38	40	41	51	54	54	—	Cloudy.	Clear.	Rain.	SW	29.6
11	41	49	50	—	56	54	51	57	Rain.	– –	– –	SW	29.3
12	45	50	50	48	49	56	48	56	Rain.	Clear.	– –	N	29.3
13	36	38	44	41	47	54½	51	49	Cloudy.	– –	Clear.	NW	29.7
14	31	38	41	44	53	55½	54	55	Cloudy.	Rain.	– –	NW	29.8
15	37	41	41	43	52	55	54	55	Rain.	– –	– –	NW	29.7
16	38	42	43	42	47	53½	51	49	Clear.	– –	– –	SW	29.9
17	37	40	37	39	51½	51	50	50	Rain. Hail.	– –	Clear.	NW	29.7
18	—	35	43	46	47	50	49	49	Showery.	– –	– –	NW	29.7
19	27	31	32	35	—	43½	37	41	Partly clear.	Rain.	– –	NW	29.8
20	25	29½	30	33	35	40	38	39	Clear.	Rain.	– –	NW	29.9
21	22	28	30	34	36	37	37	38	Clear.	– –	– –	NNW	30.2
22	25	27	29	29	40	40½	45	43	Partly clear.	Showery.	Cloudy.	W	

TABLE

TABLE VII.

Thermometers at different ſtations compared together.

1785	Night.				Day.				Morning.	Afternoon.	Evening.	Wind.	Barometer.
	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.	At Chiſlehurſt.	In the garden.	On the tower.	At the ſea-ſhore.					
June	°	°	°	°	°	°	°	°					In.
1	46	47	46	47	61	65	59	61	Showery.	– –	– –	W	29.8
2	47	47	48	50	61	65	61	62	Clear.	Cloudy.	– –	N	29.8
3	49	46	47	46	74	71	67	69	Partly clear.	– –	– –	SW	29.9
4	45	42	46	44	71	70	66	69	Partly clear.	– –	Clear.	SW.NE	29.8
5	52	54	54	54	64	68	60	66	Partly clear.	Cloudy.	Rain.	W	29.7
6	48	51	49	51	63	67	57	61	Cloudy.	– –	– –	S. N	30.0
7	43	43	45	45	63	62	58	60	Cloudy.	– –	Rain.	SES	30.0
8	53	53	52	54	67	73	66	70	Rain.	– –	Showery.	SSW	30.0
9	55	56	55	55	75	75	70	73	Partly clear.	– –	– –	SW	30.2
10	50	56	55	52	82	78	74	86	Clear.	– –	– –	W	30.3
11	54	54	57	57	78	84	74	85	Clear.	– –	– –	SW	30.4
12	56	53	57	55	76	71	63	84	Clear.	– –	– –	SW.NE	30.3
13	49	41	49	—	77	67½	64	—	Clear.	– –	– –	SW.NE	30.2
14	46	42½	50½	47	71	69	64	68	Clear.	– –	– –	SW.NE	30.2
15	51	48	53	51	79	78	73	76	Clear.	– –	Hazy.	SW.NE	30.0
16	51	54	54	50	63	68	60	71	Clear.	– –	Rain.	W	29.9
17	47	48	45	47	63	61	60	61	Showery.	– –	Clear.	N	29.8
18	48	50	49	51	66	64	61	64	Cloudy.	Partly clear.	– –	N	30.2
19	44	48	48	50	69	61	57	63	Cloudy.	– –	Clear.	NE	30.3
20	42	45	48	51	69	65	60	61	Cloudy.	Clear.	Cloudy.	NE	30.3
21	52	51	49	50	67	64	59	60	Cloudy.	– –	– –	NE	30.3
22	42	47½	44	51	70	64	60	62	Cloudy.	– –	– –	NEN	30.3
23	48	51	49	50	0	61	57	63	Miſty rain.	– –	Cloudy.	NE	30.3
24	44	52	51	54	69	62	58	60	Cloudy.	Clear.	– –	NE	30.4
25	45	40	48	53	70	63	60	65	Cloudy.	– –	Clear.	NE	30.3
26	47	44	49	52	75	68	63½	64	Cloudy.	Clear.	– –	SW.NE	30.2
27	51	43	52	51	71	67	61	63	Clear.	Partly clear.	Cloudy.	SW.NE	30.[illegible]
28	53	56	55	56	72	70	66	70	Partly clear.	– –	Clear.	SW.NE	29.9
29	50	49½	55	55	75	72	68	66	Clear.	– –	– –	SW NE	29.9
30	49	50	55	57	81	75	69	67	Clear.	– –	– –	SW.NE	29.8

TABLE VIII.

Thermometers at different ſtations compared together.

	Night.				Day.		
	In the garden.	On the tower.	At the ſea-ſhore.		In the garden	On the tower.	At the ſea-ſhore.
	°	°	°		°	°	°
1784, Dec. 10	9	15	14		21	20	27
11	5	12	12		25	22	29
25	17	21	21		33	32	33
26	23	27	27		36	35	32
1785, Feb. 7	25	28½	28		38	38	37
18	15	17	13	A little ſnow fell on theſe days.	30	31	30
21	10	15	13		38	36	36
22	17	21	16		32	33	38
25	27	31	31		44	41	40
Mar. 1	18	21	20		32	31	32
3	23	25	25		36	35	36
6	25	30	30		38	39	38

IX. *Obſervations on the Manner in which Glaſs is charged with the electric Fluid, and diſcharged. By* Edward Whitaker Gray, *M. D. F. R. S.*

Read January 17, 1788.

DR. FRANKLIN, in various parts of the firſt volume of his Experiments and Obſervations, aſſerts, That the natural quantity of electric fluid in glaſs cannot be increaſed or decreaſed; and that it is impoſſible to add any to one ſurface of a plate or jar, unleſs an equal quantity be, at the ſame time, given out from the other ſurface [*]. This error has been adopted by ſucceeding electricians; among others, by the late Mr. HENLY, who in one of his laſt Papers, printed in the Philoſophical Tranſactions for the year 1777, has the following words: " According to Dr. FRANKLIN's theory, " the ſame quantity of the electric matter which is thrown " upon one of the ſurfaces of glaſs, in the operation of charg- " ing it, is at the ſame time repelled or driven out from the other " ſurface; and thus one of the ſurfaces becomes charged plus, " the other minus; and that this is really the caſe is, I " think, ſatisfactorily proved, &c. [†]."

BECCARIA alſo has adopted the ſame opinion, ſaying, " That " a quantity of exceſſive fire cannot be introduced into one

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[*] " The quantity proportioned to glaſs it ſtrongly and obſtinately retains, and " will have neither more nor leſs." Experiments and Obſervations, Vol. I. p. 26. See alſo p. 75 81. et alibi.

[†] Philoſophical Tranſactions, Vol. LXVII. p. 100.

" ſurface, but inaſmuch as an equal doſe of natural fire can
" quit the other ſurface*."

Theſe aſſertions are, I apprehend, directly contrary to what really happens. Inſtead of which, I believe, we may ſafely aſſert, that glaſs, and every other known ſubſtance, may have its natural quantity of electric fluid either increaſed or diminiſhed to a certain limited degree; which degree bears no proportion to the quantity of matter contained in a body, but is (*cæteris paribus*) in proportion to the extent of its ſurface.

This law, which is, perhaps, without exception, may be conſidered as one of the fundamental laws of electricity, and one upon which many of its principal phænomena depend. At preſent I ſhall only conſider it ſo far as it is the cauſe of what is commonly called the charge of a coated jar.

Suppoſe ſuch a jar inſulated, and connected by its knob to the prime conductor of an electric machine; if then the machine be put in action, a certain quantity of electric fluid (agreeable to the above-mentioned law) is added to the natural quantity belonging to the inner ſurface of the jar. After which, if the finger, or any other conducting ſubſtance, be preſented to the outer coating of the jar, a quantity of electric fluid, nearly equal to that thrown in, comes from it. But this departure of electric fluid from the outſide of the jar, cannot be (as Dr. FRANKLIN ſuppoſes it) the cauſe which permits the addition of fluid to the inſide, but is merely the conſequence of the action of that ſuperfluous quantity which was thrown in. And the operator may, if he pleaſes, inſtead of taking electric fluid from the outſide of the jar, take out again (by touching the knob) nearly the whole of what he had thrown in, which he could not do if an equal quantity had already gone from the outſide of the jar *.

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* Treatiſe upon Artificial Electricity. Eng. Tranſ. ſect. 181.

When the quantity already ſpoken of has been taken from the outſide of the jar (the equilibrium being nearly reſtored) another quantity like the firſt may again be added to the inner ſurface: after which a ſimilar quantity may again be taken from the outſide: thus, by the ſucceſſion of a ſufficient number of the quantities allowed by the before-mentioned law, the jar may, at length, be completely charged.

There are other ways of charging coated glaſs; but if it be allowed, that the charge, in the foregoing inſtance, is produced in the manner I have ſuppoſed, it will not, I think, be diſputed, that all other charges are produced by a ſimilar alternation of ſmall quantities. This, however, will appear more clear from the obſervations I ſhall now make on the manner in which the diſcharge is produced.

When the aſtoniſhing velocity with which the charge of a jar or battery moves through a conſiderable ſpace is conſidered, it may at firſt appear impoſſible, that the diſcharge ſhould be made by the alternate giving and receiving ſuch ſmall quantities as thoſe by which the charge was produced; yet a more ample conſideration of the matter will, I think, ſhew that it cannot poſſibly be brought about any other way.

I preſume it will be granted, that the charge of a jar (in diſcharging) either leaves it all at once, or goes out by the ſame ſmall quantities by which it went in. To ſuppoſe any inter-

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* Much diſpute has ariſen among electricians reſpecting the degree of charge which may be given to an inſulated jar; but no one, that I know of, has taken notice of a deception which will happen, if care be not taken that the ſame ſide, by which the jar is attempted to be charged, be firſt touched in trying whether it be charged or not; whereas it is clear, from what has been ſaid, that, if the contrary ſurface be firſt touched, a ſmall charge will, from that very circumſtance, be produced.

mediate manner would neither leſſen the difficulty, nor would it be conſonant to any of the known laws of electricity.

If then the whole charge leave the jar all at once, there muſt be a point of time at which the jar will be without any electric fluid either on one ſide or the other: nay more, ſuppoſe a large jar or battery to be diſcharged by means of a few inches of thin wire, there will then be a point of time at which the whole quantity of electric fluid, which conſtituted the charge, muſt be contained in a piece of wire, weighing only a few grains.

Now, if it be conſidered, that time (like matter) is infinitely diviſible, may we not rather ſuppoſe, that the diſcharge of a jar is nothing more than an inconceivably rapid ſucceſſion of ſuch ſmall quantities as may be ſent off, without cauſing ſuch a deſtruction of the equilibrium as the laws of electricity ſeem not to admit?

That this ſuppoſition is not quite free from objections I readily admit; but before they are permitted to overthrow it, let it be well conſidered, whether they are (upon the whole) as ſtrong as thoſe I have ſtated againſt the oppoſite opinion, which I think may be pronounced to militate not only againſt what has been here mentioned as a fundamental law of electricity, but alſo againſt every known fact.

X. *Experiments on the cooling of Water below its freezing Point.* *By* Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read January 31, 1788.

WHEN the experiments for determining the degree of cold at which quickſilver becomes ſolid, related in the Philoſophical Tranſactions for 1783, were under conſideration, no difficulty occurred in explaining the phænomena that had been obſerved, except in the few inſtances where the mercury in the thermometer congealed, whilſt it was ſurrounded with ſome of the ſame metal in a fluid ſtate. The well-known property of water, that under different circumſtances it will bear to be cooled ſeveral degrees below its freezing point without congealing, afforded from analogy the moſt probable ſolution of this difficulty; but as neither the cauſe of that property had been inveſtigated, nor the circumſtances by which it is modified had been aſcertained, I was led to attempt ſome expements on the ſubject; not only in hopes of elucidating the above-mentioned phænomenon of the quickſilver, but alſo becauſe this very quality in water was itſelf a curious ſubject of reſearch.

I began with endeavouring to determine, whether this property belongs to it as pure water, or depends upon extraneous admixtures. For that purpoſe I poured ſome clean diſtilled water into a common tumbler glaſs, till it reached two or three inches above the bottom, and then ſet the glaſs in a frigorific mixture, made with ſnow and common ſalt. This was the method I uſed in moſt of the following experiments, ſometimes employing ice inſtead of ſnow, ſubſtituting a glaſs jar or cylinder inſtead of a tumbler, and filling the veſſel to a greater or leſs height above the bottom. I found that, in the frigoric mixture, the diſtilled water readily ſunk many degrees below 32°, ſtill continuing fluid; and by repeating the experiment with care, I ſeveral times cooled it to 24°, 23°½, and even almoſt to 23°. The temperature was aſcertained by means of a ſmall thermometer with a ſliding ſcale; and though the water was not of the ſame degree of cold throughout, yet the difference, when the experiment had been well conducted, was not conſiderable; and I was particularly careful that the thermometer ſhould not touch the ſides or bottom of the glaſs, ſo as to be affected immediately by the cold of the mixture. From theſe experiments therefore it ſeemed evident, that the property of being cooled below the freezing point did not depend on extraneous admixture, eſpecially as I found, by comparative trials, that common pump-water would ſcarcely ever bear to be cooled ſo much. An ambiguity, however, ſtill remained, on account of the air which is always mixed with water that has lain expoſed to the atmoſphere. In order to determine what might be aſcribed to this circumſtance, I put ſome of the ſame diſtilled water over the fire to boil, in a clean ſilver veſſel, and kept it in violent ebullition for a conſiderable time. In a few minutes after it had been taken off the fire, and before it was nearly cold, I ſet it in the frigorific mixture after the uſual manner; when, inſtead of freezing more readily, it bore to be cooled two degrees lower than I had ever been able to reduce the unboiled water, not congealing till the thermometer in it had ſunk to 21°. Subſequent experiments were attended with a ſimilar reſult, and have ſufficiently convinced me, that, other things equal, boiled water may be cooled a greater number of degrees below the freezing point, without congealing, than water which, not having undergone that operation, retains the air it naturally imbibes.

As a further proof that the preſence of an aërial fluid in water rather leſſens than increaſes its quality of being cooled below the freezing point, I found that diſtilled water, which had been for that purpoſe impregnated with fixed air, generally ſhot into ice at a leſs degree of cold than the ſame water in its ordinary ſtate. I ſuſpect, however, that it is uſually by the admixture of other aërial ſubſtances, ſuch as dephlogiſticated air, phlogiſticated air, or perhaps both, and not of fixed air, that water is inclined to congeal ſoon after it has paſſed the freezing point; for, as will be ſeen hereafter, *acids* rather improve than diminiſh the quality in water of reſiſting congelation.

To determine the effect of other extraneous ſubſtances, I took ſome very hard pump-water, ſuch as is found in the northern parts of London, and ſet it in the frigorific mixture. In general it congealed ſooner by one or two degrees than unboiled diſtilled water; that is, at 25° or 24° of the thermometer; and as there was ſome variation in this reſpect, I was led to remark, that the greateſt cooling uſually took place when the water was moſt clear and tranſparent. With a view to this circumſtance I took ſome New-River water, which happened at that time to be conſiderably turbid, and tried it in the frigorific mixture; when I found, very unexpectedly, that it was not in my power to cool any of it below the freezing point; a cruſt of ice always forming round the ſides and at the bottom of the veſſel, whilſt the thermometer, ſuſpended about the

middle

middle of the water, was two or three degrees above 32°. To try how far this depended on the foulneſs of the water, I collected ſome of the muddy ſediment which had been depoſited from the New-River water, and added it to the pump-water, which had before born to be cooled to 24° or 25°, ſo as to render it turbid; when it congealed, in the ſame manner as the New-River water had done, before the thermometer in the middle of it came to the freezing point. It muſt not however be imagined, that water thus made turbid is incapable of being cooled below 32°, without freezing: I have ſince repeated the experiments, with more caution in conducting them, and reduced it two or three degrees below the point of congelation. But ſtill they have all confirmed the general fact, that ſubſtances which leſſen the tranſparency of water, render it at the ſame time much more difficult to be cooled below the freezing point, and diſpoſe it to ſhoot into ice more readily, after it has paſſed that point, than pure water would do. It ſeems to be of little conſequence what the ſubſtance is that renders the water turbid: ſmall particles of any kind floating through it, I believe, have this effect, which does not take place, or at leaſt to the ſame degree, when the extraneous ſubſtance has ſubſided to the bottom.

It is this circumſtance, I ſuppoſe, which gave riſe to the opinion, that boiled water freezes ſooner than unboiled: for if the water contain calcareous earth, held in ſolution by means of fixed air, as is the caſe with moſt kinds of ſpring-water, this will be precipitated by the boiling, and will ſenſibly trouble the tranſparency of the water; which, if expoſed to the cold in that ſtate, will be liable to freeze ſooner than the ſame kind of water unboiled and tranſparent *.

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* See Philoſophical Tranſactions, Vol. LXV. p. 124.

The effect of this want of tranſparency was very different from that of chemical mixture, as appeared by ſubſequent experiments.

Though the property of being cooled below the freezing point appeared to belong eſſentially to water in its pure ſtate, it was probable that it would be in ſome meaſure altered or modified by the various ſubſtances which are capable of being diſſolved in, or chemically combining with, the water. But here a further circumſtance came to be conſidered. It is well known, that ſuch ſubſtances, uniting with water, have a power of lowering its point of congelation a greater or leſs number of degrees, according to the nature and quantity of the ſubſtance employed. The firſt object, therefore, was to determine in what manner the property of bearing to be cooled would be affected with regard to that new point of congelation. For this purpoſe I made many experiments with ſeveral different ſubſtances, which it would be too long to relate in detail, but the principal were as follows.

Having diſſolved in diſtilled water as much common ſalt as lowered its freezing point to 28°, I cooled it to 18°½ before it congealed. Another ſolution of the ſame ſalt, whoſe freezing point was 16°, bore to be cooled to 9°; and a ſtronger ſolution, whoſe freezing point was 13°½, cooled to 5° before it ſhot. A ſolution of nitre, whoſe freezing point was 27°, cooled to 16°, that is, eleven degrees below its new freezing point; a ſolution of ſal ammoniac, whoſe freezing point was 12°, cooled to 3°; and one of Rochelle ſalt, freezing point 27°½, ſuffered the thermometer to ſink in it to 16° before it froze; a cooling equal to the greateſt I ever obtained with the pureſt diſtilled water boiled. A ſolution of green vitriol, whoſe freezing point was near 30°, cooled below 19°: and, of ſalts with an earthy baſis, a ſolution of the common bitter purging ſalt, whoſe freezing point was at 25°½, bore to be cooled to 19°.

Acids, as I have already had occaſion to remark, rather augment this quality of being cooled below the freezing point. A combination of nitrous acid with diſtilled water, in ſuch proportions that the new freezing point was between 18° and 19°, ſunk down to 6° before it congealed; which being fully 12 degrees of cooling, is greater than I have been able to produce with pure water. Another mixture of the ſame kind, ſo ſtrong as to have its freezing point about 11°, cooled down to 1°. A mixture of vitriolic acid and diſtilled water, whoſe freezing point was 24°½, cooled to 14°; and one with the acid of ſalt, having its freezing point at 25°, ſunk to 16° before it froze. It is here to be obſerved, that theſe acid mixtures were rather remarkable for the ſteadineſs with which they bore to be cooled, and the little tendency they ſhewed to ſhoot before they were ſunk much below the freezing point, than for exceeding the number of degrees which pure water might be cooled. Of the alkalies, a ſolution of tartar, whoſe freezing point was 25°½, cooled to 18°; and another, with the freezing point at 15°, ſunk to 8°. A ſolution of cryſtalliſed ſoda, freezing point 30°, cooled to 21°; and a ſolution of mild volatile alkali, freezing point 19°, to 11°. A mixture of rectified ſpirit of wine and water, whoſe freezing point was 12°, cooled to 5°; and another, with the freezing point at 8°½, to 2°.

All theſe facts, with many others of the ſame nature which I obſerved, ſufficiently ſhew, that foreign ſubſtances, chemically combined with or diſſolved in water, do not take away its property of being cooled below its point of congelation; though, by depreſſing that point, they alter the degree of cold at which the property commences. The experiments ſhew, that in ſome caſes the mixed water bore to be cooled as much below its new freezing point, as pure water below 32°; and with regard to the others, I think the variation was no greater than uſually takes place with different portions of common water. Scarcely any, perhaps none, of the above-mentioned points, were abſolutely the loweſt to which the ſolutions or mixtures could have been reduced, if the experiment had been conducted ſtill more ſlowly and cautiouſly. But however much they might all poſſibly have born to be cooled, a great difference occurred among them in the eaſe with which the operation ſucceeded. For inſtance, the ſolutions of nitre, and of Rochelle ſalt, would hardly ever ſhoot till they were cooled many degrees below their reſpective freezing points, however negligently the operation was conducted; whereas thoſe of common ſalt, ſalt of tartar, and ſome others, required conſtant attention to keep them from freezing, as ſoon as they were got four or five degrees below the point of congelation. Their difference in this reſpect may depend in part upon ſomething unknown in the nature of each particular ſalt; but there was one circumſtance to be diſtinctly traced, correſponding to what had been obſerved with pure water; namely, that the moſt tranſparent, moſt limpid ſolutions, were thoſe which admitted of being cooled with the greateſt eaſe and certainty. The ſame obſervation holds good with regard to the mixtures: thus the rectified ſpirit I employed aſſumed ſomething of an opaline tinge upon being mixed with the water (from the ſeparation of an oil it contained) and the compoſition bore to be cooled but ill, though a film of oil, regularly ſpread on its ſurface, would rather have retarded the freezing; and the acid mixtures, which cooled ſo remarkably well, ſtruck the eye particularly on account of their very perfect and uniform tranſparency. In this laſt caſe, perhaps, another circumſtance might contribute to the eaſy cooling, that the acids, by combining with the water, ſeemed to expel the air it contained more perfectly than moſt other ſubſtances, as appeared from the innumerable ſmall bubbles that were almoſt immediately formed.

Want of tranſparency, however, is only one among ſeveral cauſes which impair the property water naturally poſſeſſes, of bearing to be cooled many degrees below its freezing point. M. MAIRAN, in his elaborate Treatiſe upon Ice, having occaſion to examine this ſubject, was led by his experiments to conclude, that the cooling of water below its freezing point depends upon reſt, and that agitation is the general cauſe by which it is brought to ſhoot into ice. In this opinion he has been almoſt implicitly followed by all the writers I have ſeen, excepting only Profeſſor WILCKE, of Stockholm *. To bring it to the teſt of experiment, I ſet in the frigorific mixture ſome diſtilled water, which by boiling had been rendered capable of ſuſtaining a cold of 21° before it froze. When this water was cooled to 22°, I agitated it, by moving the tumbler, by ſhaking a quill in it, and by blowing on it ſo as to ruffle the ſurface; but it ſupported all theſe trials without congealing, and did not ſhoot till a minute or two afterwards, when by continuance in the frigorific mixture it was cooled down to 21°. In other experiments, however, all the above-mentioned kinds of agitation made ſimilar water inſtantly congeal, even when not cooled ſo low by ſeveral degrees. The congelation, therefore, muſt in theſe caſes have depended on ſome further circumſtance than the mere want of reſt. One that I ſuſpected is a ſort of tremulation, rather agitating ſmall portions of the

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* Kongl. Vetenſk. Acad. Handlingar, Vol. XXX. p. 103. 105.

water ſeparately, than moving the whole together. I have found, that ſtriking the bottom of the tumbler againſt a board would produce inſtant congelation, when ſtirring the water, or ſhaking the tumbler in the hand, would have no effect. In like manner, when in ſtirring the cooled water the quill, or ſtick of glaſs, employed for that purpoſe, ſtrikes againſt the ſide or bottom of the tumbler, the water, which had reſiſted the general ſtirring, is often by this percuſſion made to freeze. The ſame effect is produced, and with leſs uncertainty, if the quill, or ſtick of glaſs, be rubbed, and as it were ground, againſt the ſide of the tumbler. But of all ſuch methods of bringing on the congelation, that which I have found to fail the ſeldomeſt, is to rub a bit of wax againſt the ſide of the tumbler under the water; a particular roughneſs in the motion is felt, with ſome ſound, approaching to a muſical tremulation, and a cruſt of ice is immediately perceived under the wax upon the glaſs. This effect of the wax I take to be mechanical, depending on its particular ſtate of conſiſtence. Wood acts in the ſame manner, though with leſs certainty; ſo does a quill, and likewiſe glaſs; but the latter, being very hard, produces the effect with leaſt certainty. It is a mechanical action upon the water in contact with the rubbing ſubſtance and the glaſs: for if the outſide of the tumbler, or any part of the inſide above the water, be rubbed, even if it be wet ſo as to communicate a ſimilar feeling of tremulation, yet ſtill the congelation is not produced.

All theſe modes of bringing on the congelation ſucceed beſt, as might be expected, in proportion as the water is more cooled below the freezing point. Unleſs the cooling amount to four or five degrees, the friction with wax is often in vain.

From the above-mentioned facts it appears, that M. MAIRAN's poſition, though not deſtitute of foundation, was enounced by him too generally, and without ſufficient preciſion. It is the natural property of water to bear to be cooled a certain number of degrees without freezing; reſt favours this property negatively, by giving it no interruption; but moſt kinds of agitation interfere with its operation to a greater or leſs degree, and ſome perhaps would prevent it altogether; whilſt others affect it ſo little, as not to ſuperinduce the congelation, even when the cooling is brought within one degree of the greateſt that the water will bear.

Whatever be the effect of agitation, there is another cauſe which much more powerfully haſtens the congelation of water. It has been long known, that when water is cooled below its freezing point, the contact of the leaſt particle of ice will inſtantly make it congeal, the glacial cryſtals ſhooting all through the liquor, from the ſpot where the ice touches it, till the whole comes up to the freezing point. Few experiments of the minute kind afford a more ſtriking ſpectacle than this, eſpecially when the water has been cooled nearly as much as poſſible below the freezing point; both from the beautiful manner in which the cryſtals ſhoot through it, and the rapidity with which the mercury in the thermometer immerſed in it runs up through a ſpace of 10 or 11 degrees, ſtopping and fixing always at 32 in pure water. If from any circumſtance, however, as a leſs cooling, or the addition of a ſalt, the ſhooting of the ice proceed more ſlowly, the thermometer will often remain below the freezing point even after there is much ice in the liquor; and does not riſe rapidly, or to its due height, till ſome of the ice is formed cloſe to its bulb; which exemplifies the

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evolution of the latent heat from the very particles that congeal.

Many of the circumſtances attending the greater or leſs cooling of water below its freezing point depend upon this principle. In a calm day, when the temperature of the air was about 20°, I expoſed two veſſels with diſtilled water to the cold; one of them was ſlightly covered with paper, the other was left open: the former bore to be cooled many degrees below the freezing point, whilſt a cruſt of ice always formed on the ſurface of the other before the thermometer immerſed in the middle of it came to the freezing point. This phænomenon, which other obſervers have remarked without being able to account for it, appears to me clearly owing to frozen particles, which in froſty weather are almoſt always floating about in the air, often perceptibly to the ſenſes. They come moſt commonly either from clouds paſſing over head, or from ſnow or hoar-froſt lying upon the earth; and when they touch the cooled ſurface of the water, inſtantly make it freeze. That the effect does not depend ſimply on the contact of cold air, is plain from the following experiment. I expoſed to the cold a glaſs jar, with ſome diſtilled water, and placed in it two thermometers; one immerſed in the water, the other ſuſpended a little above its ſurface, in the empty part of the jar. The latter ſunk faſter than the former; but after a certain time, the thermometer above the ſurface was at 25°, and that in the water at 25°½, yet the water continued unfrozen. I perceive too by M. Wilcke's experiments, that in much more intenſe cold than we uſually experience in this country, veſſels of water ſtanding within doors in a laboratory are often cooled ſo far below the freezing point as to become almoſt full of ice upon being made to ſhoot, though the ſurface of the water be in no wiſe defended from the cold air of the laboratory. Oil ſpread over the ſurface of water has been found to prevent it from freezing, when other water ſimilarly expoſed has had a cruſt of ice formed upon it. This I aſcribe entirely to the prevention of frozen particles from coming in contact with the water: for in experiments with frigorific mixtures, in a room of moderate temperature, I do not find that oil on the ſurface has any ſenſible effect in enabling water to ſupport more cold, unleſs, indeed, where the operation is otherwiſe too much precipitated. Alſo a crack in the tumbler containing the water prevents it from cooling below the point of congelation, a thin film of ice inſinuating itſelf through the crack into contact with the water. And often, in experiments with frigorific mixtures, the congelation is brought on by raiſing the immerſed thermometer a little out of the water, and lowering it down again; ſome of the adhering water having frozen upon its ſtem.

Several other circumſtances, though not ſo diſtinctly aſcertained as the preceding, appear to facilitate the congelation of cooled water. For inſtance, in experiments with frigorific mixtures, if the cold be very intenſe, the water freezes almoſt immediately round the ſides of the veſſel, as if ſomething depended on too ſudden a change of temperature. Accordingly, the only way of inſuring the greateſt degree of cold in water without freezing, is to cool it in a very gradual manner, keeping the cold of the frigorific mixture regularly only two or three degrees below that of the water. Sudden cooling, therefore, may be conſidered as one of the cauſes which haſten congelation. No doubt this will ſometimes depend on ſuch a cold as water cannot reſiſt without freezing, being propagated through the glaſs to the neareſt part of the water, quicker than it can be diſtributed to the reſt of the water; but, I think, the above-mentioned effect takes place when no part of the fluid can be ſuppoſed to be many degrees below the freezing point.

It has been alledged, that metal in contact, either with the outſide of the veſſel containing the water, or with the water itſelf, diſpoſes it to freeze ſooner after it is cooled below 32°. Though upon repeating this experiment I have found it poſſible to cool water in a metal veſſel many degrees below its freezing point, and even to touch it, when ſo cooled, with metal equally cold, without producing congelation; yet the metal certainly tends to haſten the freezing, and, I believe, on the above-mentioned principle of too quick a change of temperature, occaſioned by its quality as a good conductor of heat. For the ſame reaſon it is more difficult to cool water much below the freezing point in thin veſſels than in thoſe whoſe bottom and ſides are of conſiderable thickneſs; the latter tranſmitting the heat more ſlowly, and allowing it thereby to be diffuſed more equably.

In cooling water below its freezing point by frigorific mixtures, it is of conſequence to keep the mixture ſome way below the upper edge of the water within the tumbler, otherwiſe the congelation quickly begins at that place. This very likely depends on the principle laſt mentioned, that the thin edge of water riſing up againſt the ſide of the glaſs, being more in contact with air than with the general maſs of water, does not ſo eaſily diſtribute its cold, and therefore ſuffers a more rapid change of temperature by the action of the mixture. Hence one of the moſt eſſential precautions for cooling water to the utmoſt without congelation, is to perform the experiment in a warm room, that the air in contact with the edges and ſurface of the water may prevent their ſudden cooling. And one of the moſt convenient veſſels for the purpoſe is a round body terminating in a neck, the body to be ſurrounded with the frigorific mixture, whilſt the water in the neck is kept above the freezing point.

Theſe are the principal facts with which my experiments have furniſhed me relative to the cooling of water below its point of congelation. I ſee no general circumſtance that applies to them all. At one time I thought that much depended on reducing the water into thin plates; an idea which was principally ſuggeſted by the more ready congelation of the edges of the water where it riſes up as a thin film on the ſides, and by the effect of extraneous ſubſtances floating in the water to haſten its freezing, which might be ſuppoſed frequently in their motions to intercept ſmall portions of the fluid, and form it into thin plates. Agitation likewiſe might act by reducing the water in ſome part or another into ſimilar plates. And as water impregnated with air appeared leſs capable of being cooled than the ſame water deprived of its air, it ſeemed not impoſſible that the air might act likewiſe by producing thin plates in different parts of the fluid. With a view to this hypotheſis I made ſeveral experiments. Into a tumbler with diſtilled water I put a quantity of ſand, which ſettled looſely to the bottom, and left the water above as tranſparent as before. This tumbler being placed in the frigorific mixture, the water bore to be cooled as well as it had done without this addition. Laying thin bits of glaſs upon one another at the bottom of the tumbler, in place of the ſand, I found no difference in the effect. By this latter experiment it was moreover proved, that points in the water do not perceptibly facilitate its cryſtalliſation into ice. Now the thin plates or wedges of water, which muſt be intercepted by the bits of ſand or glaſs, ſeem very analogous to ſuch as may be produced by floating particles of extraneous matter; and therefore the effect of the latter is probably to be aſcribed to ſome other cauſe, ſuch as haſtening the cooling, or rendering it leſs uniform, or, perhaps, communicating motion to the fluid.

In the preceding inſtances, however, the water may be conſidered as conſtituting, by its adheſion, a ſort of continued body with the ſubſtance it touches; and it might be ſuppoſed that, if one or both ſurfaces of the thin plate were in contact with air only, the congelation would take place. I therefore put down ſome air, retained by a ſmall diſh of wax, into the water to be cooled, and let it ſtay there till the whole was reduced many degrees below the freezing point; but no congelation enſued: I then contrived to turn up the diſh of wax in the water, ſo that the air it had carried down, eſcaping from under it, aſcended as a bubble to the top of the water, and there burſt: ſtill the water retained its fluidity, notwithſtanding the contact of a looſe bubble of ſuch cold air, and the motion produced by its exploſion on the ſurface. In other experiments, I have indeed ſeen the burſting of a bubble on the ſurface apparently bring on the congelation; but ſeemingly from the agitation it occaſioned. And though a wetted ſurface, or a ſmall thin portion of water, freeze in general more readily than a larger maſs, yet I have ſeen inſtances of a jar cooled many degrees below the freezing point, and the water and air in it equally cold, which had, notwithſtanding, upon its inſide, from the ſurface of the water to an inch or more above it, an evident dew that remained quite fluid, and rendered the ſurface of the glaſs wet. Under the ſame circumſtances alſo, I have raiſed the thermometer out of the cooled water, without occaſioning any congelation in the adhering thin film of that fluid. Theſe facts, though they do not ſhew but that a film of water, in contact on both ſides with air below its freezing point, would at once congeal, yet, I think, are ſufficient to prove, that its formation into thin plates is not a general cauſe of its early freezing.

From a conſideration of the above-mentioned cauſes of freezing, and their various exceptions, I am led to think, that the matter in queſtion depends upon ſome circumſtance of the more intimate nature or compoſition of the water; for inſtance, the arrangement, attractions, and perhaps ſhape, of its particles. If we ſuppoſe the particles of water to poſſeſs a kind of polarity, that is, to have particular attracting points or ſurfaces, properly arranged, not only its cryſtalliſation in regular angles, but likewiſe moſt of the above-mentioned phænomena, admit of ſome kind of explanation. For latent heat, be it a matter or motion, may be conſidered as a cauſe either leſſening the power or impeding the operation of this polarity; the effect of which is gradually diminiſhed by external cold, till at length the polarity entirely overcomes the reſiſtance it occaſions, and the attracting points or ſurfaces ruſh together. Whatever tends, therefore, to bring theſe particles into a ſtate more advantageous for their junction, as by preſenting their attracting ſurfaces more directly to one another, forcing them nearer together, or removing attractions of a contrary tendency, and allowing the particles free ſcope to follow the impulſe of their polarity, muſt tend to haſten the congelation. When particles of water, already frozen, are preſented to other fluid water of a proper degree of coldneſs, not only the attracting ſurfaces will be in the moſt favourable poſition from the arrangement they have taken in the freezing, but very poſſibly their power may be ſtronger from their union with one another; and hence ſuch water is inſtantly brought to freeze. Indeed it is this circumſtance which conſtitutes the freezing point; for that point is evidently nothing elſe but the degree of cold which renders the particles of any fluid incapable of reſiſting the attractive power of other particles of the ſame fluid, already reduced into a ſolid form: and the fact, that the cold of the freezing point is leſs than the fluid will otherwiſe bear, ſeems a proof that when the particles have acquired this arrangement of ſolidity, their attractive powers are the ſtrongeſt; ſo that the difference between the freezing point, and the greateſt cold the fluid will bear, may be conſidered as the meaſure of this additional attractive force.

Agitation may be eaſily conceived, by the various motions impreſſed upon the particles, to occaſion ſome of them to apply their polar points in a more advantageous poſition, or even to force them nearer together; and theſe effects are more likely to be produced by an intimate agitation, than by a general motion of the whole maſs. The want of tranſparency, in certain caſes, as in ſome ſolutions of ſalts, ſeems not owing to the preſence of foreign matters, but rather to depend upon a particular arrangement of the combined particles, which may diſpoſe thoſe of the water to join more readily, and detach thoſe of the ſalt. Extraneous ſubſtances, beſides their indirect effect, may, by various chances attending their floating in the water, throw the particles into favourable ſituations; and if thin plates are more diſpoſed to freeze, it may be, that the particles of water in ſuch are more free from counteracting attractions.

Sudden cooling may promote congelation ſimply by occaſioning the water at the bottom and ſides of the veſſel to acquire a greater degree of cold than the reſt. But perhaps it may have alſo another effect, admitting of a particular explanation.

nation. Water in freezing undergoes a conſiderable expanſion. This may be aſcribed to ſuch a form of its particles, and poſition of their poles, as ſhall make them, when touching and adhering by thoſe poles alone, intercept very large interſtices, which may be conſidered as the *pores* of the ice. Various poſitions of the poles and figures of the particles may be conceived, which ſhould cauſe them to occupy more ſpace, when touching in certain points only, than they filled when lying near without any contact. But in whatever way the expanſion is produced, experiment hath ſhewn that it begins ſome time before congelation; ſo that when water is cooled down to 32°, it is already ſenſibly expanded; and if the congelation does not take place here, this expanſion augments, in proportion as the water is further cooled*. The expanſion, therefore, being ſo evidently an approach to freezing, may be conſidered as an indication that the polarity already prevails ſo far as to draw the particles ſomewhat out of the ſituation they naturally aſſume in the higher temperatures. And it is conceivable, that if this operation go on very quick, and the conſequent change of poſition in the particles be made with ſome degree of velocity, they may acquire a ſmall momentum of motion, enabling them to overcome a reſiſtance which would otherwiſe prevent their junction.

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* In experiments where the water has cooled much below its freezing point, I have ſeen the expanſion ſo great as to bear a conſiderable proportion to the whole expanſion produced by freezing, which laſt, I believe, is more than one-ſeventh of the volume of the water. It ſeemed to me as if the expanſion proceeded in an increaſing ratio, being much greater upon the laſt degrees of cooling than it was upon the firſt. The difficulty of procuring a proper apparatus for theſe experiments has hitherto prevented me from aſcertaining the quantities with preciſion.

As chemical combinations all depend upon attractions between the ſubſtances which unite, it is not difficult to conceive, that a particle of ſalt, an acid, or the like, may attract a particle of water in ſuch a manner as ſhall oppoſe or diminiſh its attraction for the other particles of water. Hence the polarity may be ſo much weakened, as not to bear the ſame proportion to the reſiſting power of the latent heat, till this alſo is diminiſhed by a greater degree of cold, which conſtitutes the new freezing point. But when, by increaſing the cold, all the powers are reduced to a ſimilar ſtate of equilibrium, exactly the ſame phænomena take place as belong to the natural freezing point of the water.

To aſſiſt the conception, I have here reaſoned upon the particles of water as ſolid, and of a determinate ſhape. But it ſeems moſt probable, that the particles of matter in general are nothing more than centres to certain attractive and repulſive powers; on which hypotheſis it may be underſtood, that if two or more of theſe central points are brought much within the limits of their reſpective attractions and repulſions, theſe powers will no longer be equal at equal diſtances from their common centre. Now ſuch a combination of central points may be conſidered as one particle of any particular matter; and the unequal diſtances from the common centre at which the attractions and repulſions are equal will define what may be called the ſhape of that particle. And if, at equal diſtances, the attraction or repulſion is much greater at one point than at another, that will conſtitute a polarity.

The greateſt cold I have been able to make water acquire without freezing, is near 12 degrees of FAHRENHEIT's ſcale below its common freezing point. Some diſtilled water was boiled about a quarter of an hour in a tin cup, and placed in the ſame veſſel, whilſt ſtill warm, in the frigorific mixture. The mixture was made to act very ſlowly, ſo that the operation continued more than an hour. When the immerſed thermometer had ſunk to 20°$\frac{1}{3}$, the water was ſtill fluid: I then ſhook it conſiderably, but no ice formed. After waiting ſome time, and finding the thermometer would ſink no lower, becauſe by the length of the proceſs the ſnow of the mixture was almoſt conſumed, I added ſome freſh materials, which could not be done without ſhaking the tin cup. Still, however, the water did not freeze inſtantly, though it ſhot as ſoon after as it can be ſuppoſed to have felt the influence of the new frigorific mixture. When this water was cooled to 24°, I tried the temperature of the air near its ſurface, and found it 34° or 35°, the experiment being performed in a room with a fire.

Another time I cooled ſome diſtilled water, covered with oil, below 21°, by ſimilar precautions.

This, however, is by no means the greateſt cooling of which water is ſuſceptible. In FAHRENHEIT's experiment, with an exhauſted globe half full of boiled rain water, it ſeems to have been cooled to 15°[*]. M. DE LUC likewiſe informs us, [†] that having filled a thermometer with ſome water he had purged of air by the means deſcribed in his great work upon the atmoſphere, he expoſed it to a cold which ſunk a mercurial thermometer to 14° of FAHRENHEIT's ſcale. The water in the thermometer continued tranſparent, and upon breaking the ball was found to be liquid, but froze that inſtant. In ſome of my experiments too with mixtures of nitrous acid and water, the liquor bore to be cooled as much as 13 degrees below its new freezing point; and it has been already obſerved, that the addition of an acid always expelled much air from the water. It is not improbable, therefore, that if water could be thoroughly purged of air, it would readily bear to be cooled 18 degrees, or more, below its freezing point, without congelation; though the deprivation of air, obtained by boiling it, is ſuch only as will barely enable it to admit a cooling of 12 degrees.

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[*] Philoſophical Tranſactions, Vol. XXXIII. p. 81.

[†] Idées ſur la Météorologie, Tom, II. p. 105.

Other fluids may bear to be cooled much more below their proper point of conſolidation. This is evidently the caſe in what Mr. CAVENDISH calls * the ſpirituous congelation of acids. Mr. M^c NAB's nitrous acid bore to be cooled from 30 to near 40 degrees below its freezing point †; and Mr. KEIR's vitriolic acid at the ſtrength of eaſieſt freezing continued fluid at 29°, though its heat became 46°½ when it began to congeal ‡. How low quickſilver may be cooled has not yet been aſcertained, but probably many degrees below – 40°.

So many of the above-mentioned facts were obſerved in the year 1783, that I then ventured to remark, that "independently of theſe circumſtances, neither ſtirring, agitation, a current of freſh air on the ſurface, nor the contact of any extraneous body not colder, would [neceſſarily] cauſe the water to ſhoot into ice, notwithſtanding the repeated aſſertions of authors to the contrary **." Similar experiments, made in the courſe of the ſucceeding winters, have confirmed in general the former reſults, and furniſhed the materials of the preceding ſheets. I am very ſenſible, that the ſubject ſtill remains involved in great obſcurity; nor ſhould I have troubled the Society with an account of experiments which leave ſo much uncertainty, had I not thought that they tended to elucidate a few points, and to correct ſome erroneous opinions. I hope that perſons inhabiting a climate more advantageous for the purpoſe, will be induced to undertake ſuch experiments in another, and probably a more ſucceſsful way, by expoſure to *natural* cold.

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* Philoſophical Tranſactions, Vol. LXXVI. p. 261.

† Ibid. p. 252.

‡ Ibid. Vol. LXXVII. p. 279.

** Ibid. Vol. LXXIII. p. 358.

XI. *Experiments and Obſervations relating to the Principle of Acidity, the Compoſition of Water, and Phlogiſton.* By Joſeph Prieſtley, *LL.D. F. R. S.*

Read February 7, 1788.

THAT water conſiſts of two kinds of air, dephlogiſticated and inflammable, is now, I believe, generally admitted as one of the moſt important, and beſt aſcertained, doctrines in chemiſtry. My own experiments having ſeemed to favour it, I made no difficulty of receiving it myſelf: but having, at the time of the publication of the laſt Volume of my Experiments, found that, in decompoſing the two kinds of air above mentioned by the electric ſpark, I got much leſs water than I expected, and, inſtead of it, a dark-coloured vapour, not eaſily condenſed, I could not help concluding that ſomething yet remained to be inveſtigated with reſpect to this ſubject, and determined, at a proper opportunity, to reſume my inquiries into it.

At that time, however, I had no ſuſpicion of any *acid* being produced in the proceſs; having never been able to find any in the water which I had hitherto procured in pretty large quantities from the decompoſition of thoſe two kinds of air, though the doctrine of dephlogiſticated air being, or containing, the principle of *univerſal acidity*, had been advanced by M. LAVOISIER, and admitted by myſelf and others.

Suſpecting that much of the water which had been procured

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in the above-mentioned proceſs was no proper conſtituent part of the air, but only ſuch as had been diffuſed through it, and in ſome manner attached to it, and kept ſuſpended in it, and therefore might be ſeparated from it, without decompoſing the air; on reſuming theſe experiments, I uſed every precaution I could think of to detach all water from the air on which I operated. In order to this, I kept it confined by mercury, together with a quantity of *fixed ammoniac*, which imbibes water more readily, if not in greater quantity, than quick-lime, or any other known ſubſtance.

In this more accurate method of making the experiment, I was gradually led to diſcover the acid, which had eſcaped my obſervation before. But I am not certain that I ſhould have found it even now, if I had not been aided by the ſagacity of Mr. KEIR, who was always of opinion, that ſome acid *muſt* be the produce of this experiment, or rather that the produce would be ſomething which would become acid by expoſure to the open air.

I began with making the exploſions in the ſame glaſs veſſel from which the mixture of air had diſplaced the mercury with which it had been filled; when I found, as I have obſerved in my laſt publication, the whole of the veſſel was filled with a denſe ſmoke, which ſettled into a black coating of all the inſide of the veſſel, and which appeared, as before, to be mercury; becoming white by expoſure to the air. For ſome time I perceived no appearance of *water:* but placing the veſſel at a proper diſtance from a fire, I found about a quarter of a grain collected on the oppoſite ſide; when, as the veſſel contained four ounce meaſures of air, the water produced ought to have been at leaſt a grain.

The mercury being an impediment in this proceſs, I afterwards confined the mixture of air in one veſſel (with mercury and fixed ammoniac as before) but I made the exploſions in another, which I had previouſly exhauſted of air. This veſſel was larger than that which I had uſed before, containing ſomething more than eight ounce meaſures; ſo that the air it contained, being one-third dephlogiſticated and two-thirds inflammable, would have weighed about two grains. After one exploſion the quantity of water collected appearing inconſiderable, I repeated the proceſs in the ſame veſſel, and then collecting the water, I found it not to exceed a grain and an half.

I repeated this experiment very often, and conſtantly found ſome water, but it always fell far ſhort of the weight of the air decompoſed. There muſt, therefore, have been ſomething not very fluid adhering to the ſides of the veſſel, which could not be diſlodged by a moderate heat; and indeed the glaſs did not recover the perfect clearneſs that it had before the proceſs.

I always obſerved, that, preſently after every exploſion, the veſſel was filled with a denſe vapour, ſo that it was ſometimes impoſſible to ſee through it; and before I admitted the external air, I could pour it from one end of the veſſel to the other, and it ſeemed to fall almoſt as faſt as a feather in a common vacuum, and in general it did not diſappear in leſs than ten minutes. I even found this denſe vapour when the mixture of air had been confined by water. The ſmell of the veſſel, after the proceſs, was that of the moſt offenſive kind of inflammable air from iron.

From theſe experiments it was ſufficiently evident, that ſomething more than *water* had been produced; and pouring into the veſſel a quantity of the juice of litmus, it was inſtantly turned to a deep red; ſo that it was equally evident, that an *acid* had been formed. In all the preceding experiments the dephlogiſticated air had been procured from manganeſe; and in all the experiments mentioned in this Paper, the inflammable air was from iron by water only.

A great number of ſtrong glaſs veſſels having been broken in theſe experiments, and ſometimes with ſome hazard to myſelf, and the quantity of air that I was able to decompoſe in them being ſmall, I next procured a *copper* veſſel, which contained about thirty-ſix ounce meaſures of air; and having now no other object than diſcovering the *kind* of acid that I had procured, I made repeated experiments in it; and after every ten or twelve exploſions collected all the liquid matter I could find; which, as the air had been previouſly confined by water, was pretty conſiderable, about equal to the weight of the air.

The liquor that I procured in this manner was always of a deep blue or green, being evidently a ſolution of copper. But it alſo contained a redundant acid, as appeared by its turning the juice of litmus red. Beſides this blue liquor, there was always a quantity of ſeemingly abraded copper; for it was perfectly and quickly diſſolved by volatile alkali, as copper very minutely divided would have been.

In theſe experiments I uſed, at different times, dephlogiſticated air from manganeſe, from red precipitate, and from red lead, as the moſt unexceptionable of all; and as it was obligingly furniſhed me by Mr. KEIR, the preparation of it may be depended upon. There did not, however, appear to be any other difference in the liquors produced by means of theſe kinds of dephlogiſticated air, except in the ſhade of the colour; that from manganeſe being of the deepeſt blue, and that from red lead the lighteſt; and this difference might be accidental.

By the aſſiſtance of Mr. KEIR I examined theſe ſolutions of copper, and preſently found, by means of a ſolution of terra ponderoſa in ſpirit of ſalt, that it was not, in any of the caſes, the vitriolic; and yet, as the dry ſubſtance left by the evaporation of the liquor did not deliqueſce, I had concluded, that the acid was neither the nitrous, nor the marine; but Mr. KEIR informs me, that this is the caſe with a fully ſaturated ſolution of copper in ſpirit of nitre.

Alſo Dr. WITHERING, who was ſo obliging as to examine ſome of theſe liquors for me (for, not being much accuſtomed to theſe analyſes, I had requeſted him to undertake it) had procured from that in the production of which the *red lead* had been uſed, cryſtals of nitre, and other indiſputable indications of nitrous acid; ſo that I was ſatisfied that it was this acid that was produced in all the caſes.

I had a farther proof of the acid being the nitrous, that having (in order to get a quantity of liquor that ſhould be as little ſaturated with any metal as poſſible) uſed a veſſel of *tinned iron*, I found, that after ſome time, when the tin had been much corroded (and with every proceſs a conſiderable quantity came away) the liquor, which at firſt was colourleſs, was tinged with red. In theſe experiments I made uſe of dephlogiſticated air from red lead.

As both the kinds of air made uſe of in theſe experiments were exceedingly pure, it ſeems evident, that phlogiſticated air does not contain all the elements of nitrous acid; but only ſupplies a *baſe* for it, the dephlogiſticated air (which was uſed in a greater proportion in the valuable experiment of Mr. CAVENDISH) ſupplying the acidifying principle, as I had conjectured in the laſt Volume of my Experiments, p. 404. Beſides, though all phlogiſticated air could not be excluded in thoſe

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experiments in which the air-pump was uſed, this objection cannot well be made to thoſe in which that inſtrument was not uſed; and in them the ſlowly condenſable vapour above mentioned ſeems to be an evident ſymptom that the produce was not mere water. But it is a ſatisfactory anſwer to this objection, from the preſence of *phlogiſticated air* in the tube, that this kind of air is not decompoſed, or at all affected, by this proceſs, as will be found by mixing any quantity of it with the two other kinds of air.

That a conſiderable quantity of water enters into the compoſition of dephlogiſticated air, will not be thought improbable, when it is conſidered that, in my former experiments, this appeared to be the caſe with reſpect to *inflammable air.* For without water this air cannot be procured. I can alſo now ſay, that the ſame is the caſe with reſpect to *fixed air.* It is not therefore improbable, that the ſame may be true of every other kind of air, ſince water is uſed in the production of them all.

Terra ponderoſa aerata (a ſubſtance of which Dr. WITHERING has given us an excellent analyſis) gives no fixed air by mere heat. But I find, that when ſteam is ſent over it, in a red heat, in an earthen tube, fixed air is produced with the greateſt rapidity, and in the ſame quantity as when it is diſſolved in ſpirit of ſalt: and, making the experiment with the greateſt care, I find, that fixed air conſiſts of about half its weight of water.

From two ounces of the terra ponderoſa I got, by means of ſteam, 190 ounce meaſures of fixed air, ſo pure that at firſt 150 ounce meaſures of it were reduced by agitation in water to 3½, and of the laſt produce, 30 ounce meaſures were reduced to one. Examining the reſiduum of the firſt portion by means of nitrous air, I found it to be of the ſtandard of 1.5.

After this, attending to the *water* expended in the proceſs, I found that I procured 330 ounce meaſures of fixed air with the loſs of 160 grains of water. According to this, as the air weighed 294 grains, the water in the fixed air muſt have been 80 parts of 147 of the whole.

In another experiment, having previouſly found that three ounces of the terra ponderoſa yielded about 250 ounce meaſures of fixed air, I attended only to the loſs of water in procuring it, and I found it to be about one-fifth of an ounce, in two ſucceſſive trials. The quantity of fixed air would weigh 225 grains, and the water expended about 100 grains; ſo that, in this experiment alſo, the fixed air muſt have contained about one-half of its weight of water.

That water enters into the compoſition of fixed air, and adds conſiderably to its weight, is farther probable from the ſolution of terra ponderoſa in ſpirit of ſalt. Becauſe when the ſolution is evaporated to dryneſs, and the reſiduum expoſed to a red heat, the weight of the air, and of this reſiduum, exceeds that of the ſubſtance from which it was procured; and it is probable, that a red heat would expel any marine acid adhering to it.

Forty-eight grains of terra ponderoſa diſſolved in ſpirit of ſalt, and then evaporated to dryneſs, and expoſed to a red heat, loſt four grains, and yielded eight ounce meaſures of fixed air, which would weigh 7.2 grains; conſequently, three-ſevenths of the weight of the air was ſomething that had been gained in the proceſs, and therefore probably water.

The near coincidence of the reſults of theſe different experiments is remarkable, and makes it almoſt certain, that no marine acid is retained in the terra ponderoſa that has been diſſolved in it, after expoſure to a red heat; that the generation of the fixed air carries off part of the water in the menſtruum; and that this part of the weight is about one-half of the whole.

I muſt obſerve, that the ſuppoſition of water entering into the conſtitution of all the kinds of air, and being, as it were, their proper *baſis*, that without which no aëriform ſubſtance can ſubſiſt (which the preceding experiments render in a high degree probable) makes it unneceſſary to ſuppoſe, as myſelf as well as others have done, that water conſiſts of dephlogiſticated air and inflammable air, or that it has ever been either compoſed or decompoſed in any of our proceſſes.

That water is decompoſed when inflammable air is procured from iron by ſteam, is not probable; ſince the inflammable principle may very well be ſuppoſed to come from the iron, and the addition of weight acquired by the iron may be aſcribed to the *water* which has diſplaced it. Alſo when the *ſcale of iron*, or *finery cinder*, is heated in inflammable air, it gives out what it had gained, *viz.* the water.

The moſt plauſible objection to this hypotheſis is, that iron gains the ſame addition of weight, and becomes the ſame thing, whether it be heated in contact with ſteam, or ſurrounded by dephlogiſticated air. But from the preceding experiments it appears, that by far the greateſt part of the weight of dephlogiſticated air is water; and the ſmall quantity of acid that is in it may well be ſuppoſed to be employed in forming the *fixed air*, which is always found in this proceſs: for that there is one common principle of acidity, and that all the acids are convertible into one another (at leaſt the nitrous acid into fixed air) is by no means an improbable ſuppoſition, though we are not yet in poſſeſſion of any proceſs by which it may be done. It is pretty evident that, in this reſpect, nature actually does what we are not able to do.

In my laſt Volume of Experiments, I recited the particulars of one, the reſult of which ſeemed to be diſſimilar to this with the ſcales of iron and inflammable air; for heating *red precipitate* in inflammable air, I then found little or no water; but having uſed more precautions, I have ſince found it in ſufficient quantity in this proceſs, even though the inflammable air was previouſly well dried with fixed ammoniac. In this experiment I diſcontinued the proceſs after three ounce meaſures of air were abſorbed, leaving room in the veſſel, that the moiſture might be more eaſily collected. With this precaution, and warming the veſſel, I collected between an half and three-fourths of a grain of water.

This experiment may be thought to be unfavourable to my preſent hypotheſis, as all water was carefully excluded, and yet a ſufficient quantity was found in the proceſs. But beſides taking into the account the water that is neceſſary to conſtitute the inflammable air, why may not *red precipitate*, in its dryeſt ſtate, be ſuppoſed to contain water, as well as the ſcales of iron, which will bear any degree of heat without parting with it. Red precipitate is made by a liquid proceſs, and therefore the water, that may enter into its compoſition as a calx, may quit it when it becomes a metal.

I ſhall take the liberty to obſerve farther, that the doctrine of the *decompoſition of water* being ſet aſide, that of *phlogiſton* (which, in conſequence of the late experiments on water, has been almoſt univerſally abandoned) will much better ſtand its ground, as all the newly diſcovered facts are more eaſily explained by the help of it.

If water be not decompoſed, both metals and ſulphur do certainly yield inflammable air, when ſteam is made to paſs over them in a red heat. They cannot, therefore, be *ſimple ſub-*

ſtances, as the antiphlogiſtic theory makes them to be. Alſo, the ſame thing that they have parted with, *viz.* inflammable air (or rather ſomething that is left of inflammable air when the water is taken from it, and which may as well be called phlogiſton as any thing elſe) may be transferred to other ſubſtances, and thus contribute to form any of the metals, ſulphur, phoſphorus, or any thing elſe that has been deemed to contain phlogiſton. This phlogiſton alſo, no doubt, having weight, it perfectly correſponds to the definition of a *ſubſtance*, having certain affinities, by means of which it is transferred from one body to another, as much as the different acids.

If there be no ſuch thing as one principle of phlogiſton, transferable from one ſubſtance to another, it muſt be admitted, that inflammable air from ſulphur is real ſulphur and water, that from iron, iron and water, as well as that very different ſubſtance, the *ſcale of iron.* And ſince copper, or any other metal, may be made of inflammable air from iron, &c. all the metals will be, in fact, convertible into one another. At leaſt, it may be ſaid, that all the component parts of any one metal may be ſo incorporated with any other, that no teſt can detect it. Alſo iron, made of inflammable air from ſulphur, ought, upon this hypotheſis, to have the properties of *ſulphurated iron*, which undoubtedly it would not have. An hypotheſis loaded with theſe difficulties muſt be inadmiſſible; whereas that of phlogiſton is extremely ſimple, and, as far as appears, of univerſal application.

The diſcovery that the greateſt part of the weight of inflammable air, as well as of other kinds of air, is water, does not make the uſe of the term phlogiſton leſs proper: for it may be ſtill given to that *principle*, or thing, which, when added to water, makes it to be inflammable air; as the term *oxygenous principle* may be given to that thing which, when it is incorporated with water, makes dephlogiſticated air.

As there is ſomething in dephlogiſticated air that ſeems to be the principle of *univerſal acidity*, ſo I am ſtill inclined to think, as I obſerved in my laſt Volume of Experiments, that phlogiſton is the principle of *alkalinity*, if ſuch a term may be uſed; eſpecially as alkaline air may be converted into inflammable air.

In the courſe of experiments recited in this Paper, I diſcovered more completely than before the ſource of my former miſtake, in ſuppoſing that fixed air was a neceſſary part of the produce of red lead, and alſo of manganeſe. Both theſe ſubſtances, I find, give of themſelves only dephlogiſticated air, and that of the pureſt kind; and all the fixed air they yielded in my former experiments muſt have come from the gun-barrel I then made uſe of, which would yield inflammable air, which, with dephlogiſticated air, forms fixed air. For though the dephlogiſticated air from red lead was ſo pure that, mixed with two meaſures of nitrous air, the three meaſures were reduced to five hundredth parts of a meaſure, and the ſubſtance gave no fixed air at all when it was heated in an earthen tube or retort; yet by mixing iron filings with it, or with manganeſe, as I had formerly done with red precipitate, I got more or leſs fixed air at pleaſure, and ſometimes no dephlogiſticated air at all.

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XII. *Some Obſervations on the Irritability of Vegetables.*

By James Edward Smith, *M. D. F. R. S.*

Read February 14, 1788.

HAVING often heard that the ſtamina of the Barberry, *Berberis communis*, were endued with a conſiderable degree of irritability, I made the experiment in Chelſea Garden, May 25, 1786, on a buſh then in full flower. It was about one o'clock P. M. the day bright and warm, with little wind.

The ſtamina of ſuch of the flowers as were open were bent backwards to each petal, and ſheltered themſelves under their concave tips. No ſhaking of the branch appeared to have any effect upon them. With a very ſmall bit of ſtick I gently touched the inſide of one of the filaments, which inſtantly ſprung from the petal with conſiderable force, ſtriking its anthera againſt the ſtigma. I repeated the experiment a great number of times; in each flower touching one filament after another, till the tips of all ſix were brought together in the center over the ſtigma.

I took home with me three branches laden with flowers, and placed them in a jar of water, and in the evening tried the experiment on ſome of theſe flowers, then ſtanding in my room, with the ſame ſucceſs.

In order to diſcover in what particular part of the filaments this irritability reſided, I cut off one of the petals with a very fine pair of ſciſſars, ſo carefully as not to touch the ſtamen which ſtood next it: then, with an extremely ſlender piece of quill, I touched the outſide of the filament which had been next the petal, ſtroaking it from top to bottom; but it remained perfectly immoveable. With the ſame inſtrument I then touched the back of the anthera, then its top, its edges, and at laſt its inſide; ſtill without any effect. But the quill being carried from the anthera down the inſide of the filament, it no ſooner touched that part than the ſtamen ſprung forwards with great vigour to the ſtigma. This was often repeated with a blunt needle, a fine briſtle, a feather, and ſeveral other things, which could not poſſibly injure the ſtructure of the part, and always with the ſame effect.

To ſome of the antheræ I applied a pair of ſciſſars, ſo as to bend their reſpective filaments with ſufficient force to make them touch the ſtigma; but this did not produce the proper contraction of the filament. The incurvation remained only ſo long as the inſtrument was applied; on its being removed, the ſtamen returned to the petal by its natural elaſticity. But on the ſciſſars being applied to the irritable part, the anthera immediately flew to the ſtigma, and remained there. A very ſudden and ſmart ſhock given to any part of a ſtamen would, however, ſometimes have the ſame effect as touching the irritable part.

Hence it was evident, that the motion above deſcribed was owing to an high degree of irritability in the ſide of each filament next the germen, by which, when touched, it contracts, that ſide becomes ſhorter than the other, and conſequently the filament is bent towards the germen. I could not diſcover any thing particular in the ſtructure of that or any other part of the filament.

This irritability is perceptible in ſtamina of all ages, and not merely in thoſe which are juſt about diſcharging their pollen. In ſome flowers which were only ſo far expanded that they would barely admit a briſtle, and whoſe antheræ were not near burſting, the filaments appeared almoſt as irritable as in flowers fully opened; and in ſeveral old flowers, ſome of whoſe petals with the ſtamina adhering to them were falling off, the remaining filaments, and even thoſe which were already fallen to the ground, proved full as irritable as any I had examined.

From ſome flowers I carefully removed the germen, without touching the filaments, and then applied a briſtle to one of them, which immediately contracted, and the ſtigma being out of its way, it was bent quite over to the oppoſite ſide of the flower.

Obſerving the ſtamina in ſome flowers which had been irritated returning to their original ſituations in the hollows of the petals, I found the ſame thing happened to all of them ſooner or later. I then touched ſome filaments which had perfectly reſumed their former ſtations, and found them contract with as much facility as before. This was repeated three or four times on the ſame filament. I attempted to ſtimulate in the midſt of their progreſs ſome which were returning, but not always with ſucceſs; a few of them only were ſlightly affected by the touch.

The purpoſe which this curious contrivance of nature anſwers in the private œconomy of the plant, ſeems not hard to be diſcovered. When the ſtamina ſtand in their original poſition, their antheræ are effectually ſheltered from rain by the concavity of the petals. Thus probably they remain till ſome inſect coming to extract honey from the baſe of the flower, thruſts itſelf between their filaments, and almoſt unavoidably touches them in the moſt irritable part: thus the impregnation of the germen is performed; and as it is chiefly in fine ſunny weather that inſects are on the wing, the pollen is alſo in ſuch weather moſt fit for the purpoſe of impregnation. It would be worth while to place a branch of the Barberry flower in ſuch a ſituation, as that no inſect, or other irritating cauſe, could have acceſs to it; to watch whether in that caſe the antheræ would ever approach the ſtigma, and whether the ſeeds would be prolific.

I have been the more particular in theſe obſervations upon the Barberry, becauſe although ſeveral authors mention the irritability of its ſtamina, none, that I can find, have related in what part of the ſtamina this property reſides, or the purpoſe it ſerves; at leaſt they have not purſued their inquiries with any great degree of accuracy, but ſeem moſtly to have copied one another. GMELIN, who has written a diſſertation expreſſly on the irritability of vegetables, has ſcarcely any thing new on the ſubject; the chief part of his work is a catalogue of plants which he found *not* to be irritable.

The Barberry is not the only plant which exhibits this phænomenon. The ſtamina of *Cactus Tuna*, a kind of Indian Fig, are likewiſe very irritable. Theſe ſtamina are long and ſlender, ſtanding in great numbers round the inſide of the flower. If a quill or feather be drawn through them, they begin in the ſpace of two or three ſeconds to lie down gently on one ſide, and in a ſhort time they are all recumbent at the bottom of the flower. The motions in *Dionæa muſcipula*, *Mimoſa ſenſitiva* and *pudica*, are too well known to be mentioned here. A ſimilar phænomenon has been obſerved, where indeed an obvious botanical analogy would lead one to expect it, in the *Droſera*.

See Dr. WITHERING's Botanical Arrangement of Britiſh Plants. All theſe movements are, I think, certainly to be attributed to irritability. We muſt be careful not to confound them with other movements, which, however wonderful at firſt ſight, are to be explained merely on mechanical principles. The ſtamina of the *Parietaria*, for inſtance, are held in ſuch a conſtrained curved poſition by the leaves of the calyx, that as ſoon as the latter become fully expanded, or are by any means removed, the ſtamina, being very elaſtic, fly up, and throw their pollen about with great force. I have lately obſerved a ſimilar circumſtance in the flowers of *Medicago falcata.* In this plant the organs of generation are held in a ſtraight poſition by the carina of the flower, notwithſtanding the ſtrong tendency of the infant germen to aſſume its proper falcated form. At length, when the germen becomes ſtronger, and the carina more open, it obtains its liberty by a ſudden ſpring, in conſequence of which the pollen is plentifully ſcattered about the ſtigma. The germen may at pleaſure be ſet at liberty by nipping the flower ſo as gently to open the carina, and the ſame effect will be produced.

As the foregoing experiments ſhew vegetables to poſſeſs irritability in common with animals, ſo there are plants which ſeem to be endued with a kind of ſpontaneous motion. LINNÆUS having obſerved that the Rue moves one of its ſtamina every day to the piſtillum, I examined the *Ruta chalepenſis*, which differs very little from the common Rue, and found many of the ſtamina in the poſition which he deſcribes, holding their antheræ over the ſtigma; while thoſe which had not yet come to the ſtigma were lying back upon the petals, as well as thoſe which, having already performed their office, had returned to their original ſituation. Trying with a quill to ſtimulate the ſtamina, I found them all quite devoid of irritability. They are ſtout, ſtrong, conical bodies, and cannot, without breaking, be forced out of the poſition in which they happen to be. The ſame phænomenon has been obſerved in ſeveral other flowers; but it is no where more ſtriking or more eaſily examined than in the Rue.

I could wiſh to find an inſtance of this ſpontaneous motion combined with irritability in one and the ſame plant; but, I confeſs, I do not know one. From analogy I ſhould think it not impoſſible that the *Dionæa muſcipula*, and perhaps the *Droſeræ*, may have the ſame motion in their ſtamina as the *Ruta*, *Parnaſſia*, and *Saxifraga*, while their leaves poſſeſs irritability. But if this be the caſe, the ſeats of theſe two properties, being ſo different and remote from each other, ſhould ſeem to have as little connexion as if in two different plants. There ſtill remains then this difference between animals and vegetables, that although ſome of the latter poſſeſs irritability, and others ſpontaneous motion, even in a ſuperior degree to many of the former, yet thoſe properties have hitherto in animals only been found combined in one and the ſame part. Even *Sertulariæ* are not an exception to this obſervation. The greater part of their ſubſtance, indeed, reſembles that of plants in being indefinitely extended, and in wanting irritability and ſpontaneous motion. But their animated flowers or polypes, in which the eſſence of their being reſides, are endued with both theſe properties in an high degree.

I know it is the opinion of ſome philoſophers that a certain degree of irritability muſt pervade every part of vegetables, as the propulſion of their fluids cannot well be conceived to be accompliſhed by any other means. In a converſation on this ſubject with the celebrated M. BONNET, of Geneva, he informed me, that he is ſtrongly of this opinion; and that he ſhould not deſpair, by throwing acid or other ſtimulating injections into the veſſels of ſome plants, of ſeeing with a microſcope at once the propulſion of the ſap, and the contractions by which it is performed. He urged me, with that amiable enthuſiaſm for which he is remarkable, to purſue the inquiry. Whether I do ſo or not, I think the idea too intereſting to be kept to myſelf, and ſhould be glad to ſee it realized by any one who has time and abilities for ſuch inveſtigations, who has accuracy and coolneſs in making his experiments, as well as fidelity and impartiality in recording them.

I cannot conclude this Paper without taking notice of another very curious property which vegetables ſeem to poſſeſs in common with animals, although certainly in a very inferior degree: I mean, that property, to uſe the words of Mr. HUNTER, who has ſtudied this principle to a vaſt extent in the animal œconomy, by which their conſtitution is capable only of a certain degree of action conſiſtently with health; when that degree is exceeded, diſeaſe or death is the conſequence. It is only by the help of this principle that I can explain why many plants reſiſt a great degree of cold for ſeveral winters before flowering; but, after that critical event, they periſh at the firſt approach of cold, and can by no art be preſerved ſo as to ſurvive the winter. But a more curious inſtance is that mentioned by LINNÆUS, without an explanation, in his Diſſertation on the Sexes of Plants, of the long duration of the piſtilla in the female hemp, while unexpoſed to the male pollen; whereas thoſe to which the pollen had acceſs immediately faded and withered away. In this caſe, I cannot help thinking, that in thoſe piſtilla on which the pollen had acted, and which conſequently had performed the function for which they were deſigned, the vital principle was much ſooner exhauſted than in thoſe which had known no ſuch ſtimulus. It is, perhaps, for the ſame reaſon that double flowers, in which, the organs of generation being obliterated, no impregnation can take place, laſt much longer in perfection than ſingle ones of the ſame ſpecies, as is notoriouſly the caſe with Poppies, Anemonies, &c. In ſingle Poppies the corolla falls off in a few hours; but in double ones it laſts ſeveral days; and this may poſſibly, combined with other obſervations, lead to a diſcovery of the real uſe of the corolla of plants, and the ſhare it has in the impregnation, about which there has yet been no probable conjecture.

XIII. *An Account of Experiments made by Mr.* JOHN M^c NAB, *at* Albany Fort, Hudſon's Bay, *relative to the Freezing of Nitrous and Vitriolic Acids. By* Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read February 28, 1788.

FROM the experiments made by Mr. M^c NAB, of which I gave an account in the LXXVIth Volume of the Philoſophical Tranſactions, p. 241. it appeared, that ſpirit of nitre was ſubject, not only to what I call the aqueous congelation, namely, that in which it is chiefly, and perhaps intirely, the watery part which freezes, but alſo to another kind, in which the acid itſelf freezes, and which I call the ſpirituous congelation. When its ſtrength is ſuch as not to diſſolve ſo much as $\frac{243}{1000}$ of its weight of marble, or when its ſtrength is leſs than ,243, as I call it for ſhortneſs, it is liable to the aqueous congelation ſolely; and it is only in greater ſtrengths that the ſpirituous congelation can take place. This ſeems to be performed with the leaſt degree of cold when the ſtrength is ,411, in which caſe the freezing point is at $-1°\frac{1}{2}$. When the acid is either ſtronger or weaker, it requires a greater degree of cold; and in both caſes the frozen part ſeems to approach nearer to the ſtrength of ,411 than the unfrozen part. The freezing points, anſwering to different degrees of ſtrength, ſeemed to be as follows.

Strength.	Freezing point.	
,54	−31½°	ſpirituous congelation.
,411	− 1½	
,38	−45¼	
,243	−44¼	aqueous congelation.
,21	−17	

As ſome of theſe properties, however, were deduced from reaſoning not ſufficiently eaſy to ſtrike the generality of readers with much conviction, Mr. M^c Nab was deſired to try ſome more experiments to aſcertain the truth of it; which he was ſo good as to undertake, and has executed them with the ſame care and accuracy as the former.

For this purpoſe, I ſent him ſome bottles of ſpirit of nitre of different ſtrengths, and he was deſired to expoſe each of theſe liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice was almoſt melted, and then again expoſe them to the cold, and, when a conſiderable part of the acid had frozen, to try the temperature a ſecond time; then to decant the unfrozen part into another bottle, and ſend both parts back to England, that their ſtrength might be examined.

The intent of this ſecond expoſure to the cold was as follows. Spirit of nitre bears, like other liquors, to be cooled greatly below its freezing point without freezing: then the congelation begins ſuddenly; the liquor is filled with fine ſpicula of frozen matter, and the ice becomes ſo looſe and porous, that, if the proceſs be continued long enough for a conſiderable portion of the acid to congeal, ſcarce any of the fluid part can be decanted: whereas, if it be heated in this ſtate till the frozen part is almoſt, but not intirely, melted, and be again expoſed to the cold, as the liquor is then in contact with the congealed matter, it begins to freeze as ſoon as it arrives at the freezing point, and the ice becomes much more ſolid and compact.

The intent of decanting the fluid part, and ſending both parts back, that their ſtrength might be determined, was partly to examine the truth of the ſuppoſition laid down in my former Paper, that the ſtrength of the frozen part approaches nearer to ,411 than that of the unfrozen; but it is alſo a neceſſary ſtep towards determining the freezing point anſwering to a given ſtrength of the acid; for as the frozen part is commonly of a different ſtrength from the unfrozen, the ſtrength of the fluid part, and the cold neceſſary to make it freeze, is continually altering during the progreſs of the congelation. In conſequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that neceſſary to make the remainder, or the fluid part, begin to freeze, or, in other words, it is the freezing point of the fluid part. This is the reaſon that a thermometer, placed in ſpirit of nitre, continually ſinks during the progreſs of congelation; which is contrary to what is obſerved in pure water, and other fluids in which no ſeparation of parts is produced by freezing.

Moreover, from the above-mentioned experiments of Mr. M^c^ NAB it appeared, that oil of vitriol, as well as ſpirit of nitre, is ſubject to the ſpirituous congelation; but it ſeemed uncertain, whether, like the latter, it had any point of eaſieſt freezing, or whether it did not uniformly freeze with leſs cold as the ſtrength increaſed. For this reaſon, ſome bottles of oil of vitriol, of different ſtrengths, were ſent, which he was deſired to try in the ſame manner as the former. This point, indeed, has ſince been determined by Mr. KEIR, who has ſhewn that oil of vitriol has a ſtrength of eaſieſt freezing; and that at that point a remarkably ſlight degree of cold is ſufficient for its congelation.

The reſult of Mr. M^{c} NAB's experiments on the nitrous acid is given in the following table.

N°	Decanted part.		Undecanted part.		Strength of the whole maſs.	Strength before ſent.	Freezing point by firſt method.	Freezing point by ſecond method.
	Quantity.	Strength.	Quantity.	Strength.				
6	– –	– –	– –	– –	– –	,561	–41,6	– –
7	1410	,445	2137	,435	,439	,437	+*1,7	– 3,8
8	1658	,390	1940	,422	,407	,408	– 3,5	– 4
9	1368	,353	2438	,416	,393	,391	– 4,5	–11
10	2206	,343	1920	,373	,357	,357	–12,5	–13,8
11	3620	,310	602	,381	,320	,320	–22,5	–23
12	2155	,276	1494	,293	,283	,280	–39,1	–40,3
13	1618	,241	1961	,235	,238	,238	–34	–32

The firſt column contains the numbers by which Mr. M^{c} NAB has diſtinguiſhed the different bottles. The ſecond and third columns contain the quantity and ſtrength of the decanted part of the liquor; and the fourth and fifth ſhew the quantity and ſtrength of the undecanted part of the liquor. The ſixth column gives the ſtrength of both parts put together, or the ſtrength of the whole maſs; and the ſeventh is the ſtrength of the ſame acid, as it was determined before it was ſent to Hudſon's Bay. The ſtrengths of the decanted and undecanted parts were found by ſaturating the liquor returned home with marble; and that of the whole maſs was inferred by computation from the quantity and ſtrength of the decanted and undecanted parts; and as the ſtrength thus inferred never differs from that determined before the liquors were ſent to Hudſon's Bay by more than $\frac{1}{100}$ part of the whole, it is not likely that the ſtrengths of the decanted and undecanted parts here ſet down ſhould differ from the truth by much more than that quantity.

The eighth column contains the freezing points found in the firſt method, or the temperature of the liquors after the haſty congelation which took place on expoſing them to the cold without any frozen matter in them; and the ninth contains their temperature after the more gradual congelation which took place when they were cooled with ſome frozen matter in them; and as the unfrozen part of the acid was decanted immediately after the temperature had been obſerved, it follows, that this column ſhews the true freezing points of the decanted liquors. In like manner the eighth column ſhews the freezing points of that part of the liquor which remained fluid in the firſt manner of trying the experiment; but as the ſtrength of this part was not determined, the preciſe ſtrengths to which theſe freezing points correſpond are unknown. Thus much, however, is certain, that theſe points muſt be below thoſe of the whole maſs, and in all probability muſt be above thoſe of the decanted liquor; as there is great reaſon to think, that the quantity of frozen matter was always leſs, and conſequently the ſtrength of the fluid part differed leſs from that of the whole maſs, in the firſt way of trying the experiment than in the ſecond.

Before I draw any concluſions from theſe experiments, it will be proper to take notice of ſome particularities which occurred in trying them.

N° 6. was made to congeal by a freezing mixture of ſnow and diluted oil of vitriol. By the time the acid was cooled to −42°, icy filaments were formed on the inſide of the phial above the acid. Ten minutes after, the acid being cooled one degree more, the phial was taken out and agitated. This mixed the icy filaments with the acid, and made it freeze, which it ſeems not to have done before, in conſequence of which its temperature roſe to − 41°½. After having melted the greateſt part of theſe filaments, and again expoſed it to the freezing mixture, ſome ſnow accidentally fell into the acid, and made an uncertainty in the freezing point, for which reaſon it is not ſet down. But as it is evident, that the quantity of congealed matter in the firſt experiment was exceſſively ſmall, the ſtrength of the unfrozen part could not differ ſenſibly from that of the whole maſs, and therefore − 41°½ is the true freezing point that anſwers to the ſtrength of ,561.

It is remarkable, that N° 8. acquired by congelation a bluiſh colour, not unlike that which the dephlogiſticated nitrous acid, in Mr. M^c^ NAB's former experiments, acquired by dilution with ſnow. It is not ſaid, how long the acid retained this colour, but it was intirely gone when the phial arrived in England. I am quite at a loſs to account for this phænomenon, and why it happened to this bottle only.

N° 12. when cooled to − 17° ſeemed to contain many icy particles; but as it afterwards bore to be cooled to − 48°, without their increaſing, we may conclude, that they were not frozen ſpirit of nitre, but only ſome heterogeneous matter ſeparated from it. A little of the congealed part of N° 8. dropped into it while at this point, made it freeze, and it roſe to − 39°.

In all the foregoing acids the ice was heavier than the fluid part, and in conſequence ſubſided to the bottom; a proof that it was the ſpirituous congelation which had taken place in them: but in N° 13. the frozen part ſwam at top, which ſhews, that the congelation was of the aqueous kind.

It may appear remarkable to thoſe who read Mr. M^c NAB's experiments, that theſe acids bore to be heated ſo much above their freezing points before the ice intirely diſſolved. N° 6. bore to be heated 18 degrees, N° 7. 13 degrees, and N° 12. 17 degrees above their freezing points, before all the congealed acid had diſappeared. But as, in order to diſſolve this congealed matter, they were brought into a room in all probability a great many degrees warmer than the points to which they were heated, ſo that the liquors heated faſt; and as during the diſſolution the ice would ſubſide to the bottom; it is not extraordinary, that the fluid part in the phial might be many degrees warmer than the frozen part, unleſs the phials were much agitated during the time, which nothing ſhews them to have been; eſpecially if we conſider the great quantity of heat which, in all probability, muſt be communicated to the frozen acid in order to melt it; and that, perhaps, the frozen acid may receive and part with its heat but ſlowly. It muſt be obſerved, that in N° 6. and 12. the frozen part might very likely be of a conſiderably different ſtrength, and in conſequence its freezing point might be ſeveral degrees different from that of the whole maſs, ſo that the temperature to which the fluid was heated, in order to melt the ice, might very likely not differ ſo much from the freezing point of the ice itſelf as is here ſet down. But this could not be the caſe with N° 7.

It muſt be obſerved, that when Mr. M^c NAB wanted to try the temperature of N° 7. after it had frozen in the firſt manner, the ſtopper ſtuck ſo tight that he was not able to remove it without warming it before the fire. The thermometer was then introduced, and ſtood ſeveral minutes therein at $+1°\frac{1}{2}$, or $+2°$. As the thermometer remained ſo long at this point, one might naturally ſuppoſe, that this was the true freezing point of the unfrozen acid. But yet, from what has been juſt ſaid, it ſeems not improbable that it may be otherwiſe, and that the true freezing point may be ſenſibly lower; for which reaſon it is marked in the table with an aſteriſk (*) as doubtful.

It was before ſaid, that the temperatures in the ninth column of the foregoing table, are the freezing points anſwering to the ſtrengths expreſſed in the third column, and that $-41^\circ\frac{1}{2}$ is the freezing point anſwering to the ſtrength of ,561; whence the freezing points determined by theſe experiments, and their reſpective ſtrengths, are as follows:

Strength.	Freezing point.
,561	−41°,6
,445	− 3,8
,390	− 4
,353	−11
,343	−13,8
,310	−23
,276	−40,3

By interpolation from theſe *data*, according to NEWTON's method †, it appears, that the ſtrength at which the acid freezes with the leaſt cold is ,418, and that the freezing point anſwering to that ſtrength is $-2^\circ\frac{4}{10}$.

In order to ſhew more readily the freezing point anſwering to any given ſtrength, I have computed, by the ſame method, the following table, in which the ſtrengths increaſe in arithmetical progreſſion.

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† Princip. Math. Lib. III. prop. 40. lem. 5.

Strength.	Freezing point.	Difference.
,568	− 45,5	+ 15,4
,538	− 30,1	+ 12
,508	− 18,1	+ 8,7
,478	− 9,4	+ 5,3
,448	− 4,1	+ 1,7
,418	− 2,4	− 1,8
,388	− 4,2	− 5,5
,358	− 9,7	− 8
,328	− 17,7	− 10
,298	− 27,7	

It was before ſhewn, that the freezing points, found by the firſt method, ought to be below thoſe of the whole maſs, and muſt, in all probability, be above thoſe of the decanted liquor. In order to ſee how this agrees with obſervation, I computed in the above-mentioned manner the freezing points anſwering to the ſtrength of the whole maſs, and compared them with the obſerved freezing points. The reſult is given in the following table.

N°	Strength of the whole maſs.	Strength of the decanted liquor.	Computed freezing point of the whole maſs.	Obſerved freezing point.	
				In firſt method.	In ſecond method.
7	,439	,445	− 3,2	+ 1,7	− 3,8
8	,407	,390	− 2,6	− 3,5	− 4,
9	,393	,353	− 3,7	− 4,5	− 11,
10	,357	,343	− 10,	− 12,5	− 13,8
11	,320	,310	− 19,9	− 22,5	− 23,
12	,283	,276	− 35,6	− 39,1	− 40,3

It may be obſerved, that the freezing point of N° 7. tried in the firſt way, is conſiderably above that correſponding to the ſtrength of the whole maſs; but as this experiment was ſhewn (in p. 173.) to be doubtful, and not unlikely to exceed the truth, we may ſafely reject it as erroneous. All the others, as might be expected, are lower than thoſe correſponding to the ſtrength of the whole maſs, and above thoſe obſerved in the ſecond manner, and therefore ſerve to confirm the truth of the above determination of the freezing points of ſpirit of nitre; and alſo ſhew, that in this acid the point of ſpirituous congelation is pretty regular, and does not depend much, if at all, on the rapidity with which the congelation is performed.

The point of aqueous congelation, however, ſeems liable to conſiderable irregularity; for N° 13. after having been expoſed to the cold, froze on agitation, the congelation, as was before ſaid, being of the aqueous kind, and the thermometer ſtood ſtationary therein at – 34°. The ice being then almoſt melted, it was again expoſed to the cold, till a good deal was frozen; but yet its temperature was then no lower than – 32°$\frac{1}{4}$, though the quantity of frozen matter muſt certainly have been much more than in the firſt trial. The fluid part being then decanted, and the frozen part melted, both were again expoſed to the cold. They both were made to congeal by agitation, and the temperature of the undecanted was then found to be – 35°, and that of the decanted part – 37°: ſo that it ſhould ſeem as if the freezing point found by the haſty congelation was always lower than that found the other way, which may, perhaps, proceed from this cauſe; namely, that when ſufficient time is allowed, the watery part will ſeparate from the reſt, and freeze in a degree of cold much leſs than what is required to produce that effect, when it is performed in a more rapid manner.

Theſe experiments confirm the truth of the concluſions I drew from Mr. M^c^ NAB's former experiments; for, firſt, there is a certain degree of ſtrength at which ſpirit of nitre freezes with a leſs degree of cold than when it is either ſtronger or weaker; and when ſpirit of nitre, of a different ſtrength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of ſtrength than the unfrozen. Likewiſe this ſtrength, as well as the freezing point correſponding thereto, and the freezing point anſwering to the ſtrength of ,54, come out very nearly the ſame as I concluded from thoſe experiments; for by the preſent experiments they come out ,418, $-2^\circ\frac{4}{10}$, and -31°, and by the former ,411, $-1^\circ\frac{1}{2}$, and -31°. But the freezing point anſwering to the ſtrength of ,38 is totally different from what I there ſuppoſed. This muſt have been owing to the ſtrength of that acid having been very different from what I thought it; which is not improbable, as its ſtrength was inferred only from the quantity of ſnow which was added to it in finding the degree of cold produced by its mixture with ſnow.

After the foregoing experiments were finiſhed, Mr. M^c^ NAB made ſome more for determining the freezing points both of the decanted and undecanted part; but for want of a ſufficient explanation of the manner in which they were executed, I have not been able to make any uſe of them. In their preſent ſtate they ſhew much appearance of irregularity; but this would very likely have been cleared up, if the circumſtances had been more fully detailed.

On the Vitriolic Acid.

An irregularity of a remarkable kind occurred in trying two of theſe acids; namely, when the undecanted part was melted and again made to congeal, its freezing point was found to be much leſs cold than that of the decanted part, and the difference was much greater than could be attributed to the difference of ſtrength. This ſeems to have happened only in the two ſtrongeſt acids, namely, N° 1. and 2. and in great meaſure confirms the ſuppoſition which I formed from Mr. M^c^ NAB's former experiments, that the congealed part of oil of vitriol differs from the reſt, not merely in ſtrength, but alſo in ſome other reſpect, which I am not acquainted with. It ſhould ſeem, however, that this property does not extend to weak oil of vitriol.

It perhaps may be ſuſpected, that this property takes place in the nitrous acid alſo, and was the cauſe of the ſlow melting of the ice taken notice of in p. 172. But I think it more likely, that that phænomenon proceeded from the cauſes there aſſigned.

Some ſmaller irregularities occurred in trying the vitriolic acid, the cauſe of which I believe was, that when this acid has been cooled below the freezing point, and begins to freeze, the congelation proceeds but ſlowly; ſo that a conſiderable time elapſes before it riſes to the true freezing point. Something of the ſame kind ſeems to take place in the nitrous acid alſo, though in a leſs degree; for the decanted liquors uſually continued to freeze and depoſit a ſmall quantity of ice, for a few minutes after they were poured off, though their cold, at leaſt in ſome inſtances, was found rather to diminiſh during that time.

It muſt be obſerved, that ſmall ſpicula of ice always came over along with the decanted liquor; and to this, in all probability, the new-formed ice attached itſelf; for otherwiſe it is likely, that no ice would have been produced.

The following table contains the ſtrength of the acids as determined before they were ſent to Hudſon's Bay, and the quantity and ſtrength of the decanted and undecanted parts when they arrived at London, and the ſtrength of the whole maſs as computed from thence. For the ſake of uniformity, I have expreſſed their ſtrengths, like thoſe of the nitrous acid, by the quantity of marble neceſſary to ſaturate them, though I did not find their ſtrength by actually trying how much marble they would diſſolve; as that method is too uncertain, on account of the ſelenite formed in the operation, and which in good meaſure defends the marble from the action of the acid. The method I uſed was, to find the weight of the plumbum vitriolatum formed by the addition of ſugar of lead, and from thence to compute the ſtrength, on the ſuppoſition that a quantity of oil of vitriol, ſufficient to produce 100 parts of plumbum vitriolatum, will diſſolve 33 of marble; as I found by experiment that ſo much oil of vitriol would ſaturate as much fixed alkali as a quantity of nitrous acid ſufficient to diſſolve 33 of marble. It may be obſerved, that the quantity of alkali, neceſſary to ſaturate a given quantity of acid, can hardly be determined with much accuracy, for which reaſon the foregoing leſs direct method was adopted; eſpecially as the precipitation of plumbum vitriolatum ſhews the proportional ſtrengths, which is the thing principally wanted, with as great accuracy as any method I know.

N°	Strength before ſent.	Decanted part.		Undecanted part.		Strength of whole maſs.
		Quantity.	Strength.	Quantity.	Strength.	
1	,977	1375	,967	3460	,963	,964
2	,918	3915	,919	1876	,905	,914
3	,846	88	,777	4915	,850	,849
4	,758	389	,710	3795	,753	,755
				547	,803	

The undecanted part of N° 4. was divided into two parts; namely, the leſs and the more congealable part; and it is the latter whoſe quantity and ſtrength is given in the laſt line.

It is well known, that oil of vitriol attracts moiſture with great avidity; and ſome of theſe acids were much expoſed to the air during the experiments made with them, and may therefore be ſuppoſed to have attracted ſo much moiſture from the air, as might ſenſibly diminiſh their ſtrength; and this ſeems actually to have been the caſe with ſome of them. But as the bottles were well ſtopped, and as, except in one acid which was the moſt expoſed to the air, the ſtrength of the whole maſs comes out not much leſs than that determined before the liquors were ſent to Hudſon's Bay, I imagine their ſtrength could not ſenſibly alter during their voyage home; and conſequently their ſtrength, at the time the laſt obſervations were made with them, could not differ much from that here ſet down.

It would be tedious to give the experiments for determining their freezing points in detail; but the reſult is as follows. The freezing point of N° 1. tried in the firſt method, was ſomewhat above + 1°, but it is uncertain how much; that tried in the ſecond manner ſeemed − 6°½. But the freezing point of the undecanted part, after having been intirely melted, and again expoſed to the cold, was +9°. It muſt be obſerved, that though this part was in all probability at firſt ſtronger than the decanted part, yet at the time its freezing point was tried, it ſeems to have become rather weaker than that, owing to its expoſure to the air. It was before ſaid, that the freezing point tried in the ſecond manner is that of the decanted liquor; ſo that the freezing point of the decanted part ſeems to have been 13 or 14 degrees colder than that of the undecanted part; though the difference of ſtrength, if there was any, muſt in all probability have tended to produce the contrary effect.

The freezing point of N° 2. tried in the firſt way, was −26°; and that tried in the ſecond was −30°, or 26°; but yet the freezing point of the undecanted part was 26 or 30 degrees higher, namely, at zero; a difference which could ſcarcely have proceeded from the difference of ſtrength.

The freezing point of N° 3. could hardly differ much from +42°; and that of N° 4. was about −45°.

It ſhould be remarked, that when this laſt acid, as well as N° 1. and 2. were expoſed to a great cold, a ſediment formed in them. This muſt have been of a very different nature from frozen acid, as appeared both from its texture, which was ſoft and mucilaginous to the feel, inſtead of being gritty as the frozen acid always was; and alſo from its being not much increaſed by an increaſe of cold; and therefore ſeems to have been ſome impurity ſeparated from the acid. The quantity was greateſt in N° 4.; but even in this, though it appeared great, it is likely that the real quantity was very ſmall.

Another bottle of acid, whoſe ſtrength was ,659, was ſent; but Mr. M^c^ NAB was not able to make this freeze.

From theſe experiments it ſhould ſeem, that the freezing point of oil of vitriol, anſwering to different ſtrengths, is nearly as follows:

Strength.	Freezing point.
,977	+ 1°
,918	− 26
,846	+ 42
,758	− 45

From hence we may conclude, that oil of vitriol has not only a ſtrength of eaſieſt freezing, as Mr. Keir has ſhewn; but that, at a ſtrength ſuperior to this, it has another point of contrary flexure, beyond which, if the ſtrength be increaſed, the cold neceſſary to freeze it again begins to diminiſh.

The ſtrength anſwering to this latter point of contrary flexure muſt, in all probability, be rather more than ,918, as the decanted or unfrozen part of N° 2. ſeemed rather ſtronger than the undecanted part; and for a like reaſon the ſtrength of eaſieſt freezing is rather more than ,846.

Mr. Keir found that oil of vitriol froze, with the leaſt degree of cold, when its ſpecific gravity at 60° of heat was 1,780, and that the freezing point anſwering to that degree of ſtrength was + 46°; which agrees pretty nearly with theſe experiments, as the ſtrength of oil of vitriol of that ſpecific gravity is ,848, that is, nearly the ſame as that of N° 3.

APPENDIX.

Tranſlation of Don Michael Rubin de Celis's *Letter to the Royal Society, relative to a Maſs of native Iron, found in* South-America. See p. 37.

ABOUT thirty years ago, the various barbarous nations who inhabited the provinces of the great Chaco Gualamba, expelled the Spaniards from thence; and ſince that time, the countries on the ſouthern part of the river Vermejo, and weſtern of the great river Paranà have been almoſt totally deſerted. The only employment of the few Indians who dwell within the juriſdiction of Santiago del Eſtero is to gather the honey and wax, with which the woods abound. Theſe Indians diſcovered, in the midſt of a wide-extended plain, a large maſs of metal, which they called pure iron; part of which projected above the ground about a foot, and almoſt the whole of its upper ſurface was viſible. Intelligence of this diſcovery was immediately communicated to the Viceroys of Peru. That ſuch a maſs of iron ſhould be found in a country where there are no mountains, nor even the ſmalleſt ſtone within a circumference of one hundred leagues, could not fail to appear extraordinary, notwithſtanding we know there are mines of pure iron in Europe. Some private perſons, at the great riſk of their lives, both from the uncertainty of procuring food or even water (of which none is to be found but what rain happens to be preſerved in ſome natural cavities of the earth), from the danger of meeting the roving Indians, from the various wild beaſts found in thoſe plains, ſuch as tygers, leopards, tapirs, from the ſwarms of poiſonous reptiles, and finally from the endleſs thickets, led on by hopes of enriching themſelves, boldly undertook the journey, to obtain ſome of the metal. They tranſmitted a part of it to Lima and Madrid, by which no other advantage was gained than to aſcertain it to be very ſoft and very pure iron. As it is forbidden by law, for political reaſons, to manufacture iron in that country, though different parts of it abound with iron mines; and as it was aſſerted, that the vein of iron extended many leagues, the viſible part being only its creſt projecting above the ground, which, when dug round, was found to meaſure three yards from N. to S. two yards and a half from E. to W. and about one-third of a yard in thickneſs; the Viceroy of the river Plata ſent me with orders to examine this diſcovery with accuracy; and, in caſe I found it a beneficial mine, that I ſhould eſtabliſh a colony there. I accordingly ſet off, well eſcorted, in the beginning of February, 1783, from Rio Salado, an ancient civilized hamlet of the Indians whom we call Vilelas, and purſued my journey in the direction E. ¼ N.E. though, upon further examination, I found that I ought to have taken my direction E. ¼ S E. both corrected.

The aſpect of the country between the river and the mine, diſtant ſeventy leagues from the ſettlement, is curious: it conſiſts of an immenſe plain, alternately intermixed with thick woods and fertile fields, forming moſt pleaſing landſcapes.

The latitude of the mine I found, by obſervation, to be 27° 28′ S.

There is not one fixed place of habitation throughout the whole country, owing to a ſcarcity of running water. That which is drunk by the honey-gatherers, who reſide there in ſmall bodies the greateſt part of the year, collecting honey in the woods, is rain water, as I have already obſerved. Theſe, and a few roving tribes of barbarous Indians, who reſemble the Tartars in their way of life, and come hither, at a certain ſeaſon of the year, from the borders of the river Vermejo, in queſt of a wild root, which they call Koruu, and which they conſtantly chew, as a remedy againſt the peſtilential air of their native country, and alſo as a preſervative againſt the bite of poiſonous reptiles, are the only people ever ſeen in thoſe pleaſant and extenſive plains.

I arrived the 15th of February at the place called Otumpa, where the maſs was found almoſt buried in pure clay and aſhes.

The exterior appearance of it was that of perfectly compact iron; but upon cutting off pieces of it, I found the internal part full of cavities, as if the whole had been formerly in a liquid ſtate. I was confirmed in this idea, by obſerving on the ſurface of it the impreſſions as of human feet and hands of a large ſize, as well as of the feet of large birds, which are common in this country. Though theſe impreſſions ſeem very perfect, yet I am perſuaded that they are either a *luſus naturæ*, or that impreſſions of this nature were previouſly upon the ground, and that the liquid maſs of iron falling upon it received them. It reſembled nothing ſo much as a maſs of dough, which, having been ſtamped with impreſſions of hands and feet, and marked with a finger, was afterwards converted into iron.

I began

I began to cut off part of it with chiſſels; and in ſeparating from the maſs twenty-five or thirty pounds, I ſpoiled all the chiſſels I had, to the number of ſeventy.

I ordered my men to dig round it, and found the under ſurface covered with a coat of ſcoriæ from four to ſix inches thick; undoubtedly occaſioned by the moiſture of the earth, becauſe the upper ſurface was clean.

Having moved it half round, by means of handſpikes, I ordered the ground under its bed to be dug to a conſiderable depth, and even blew it up in two places with gunpowder; after which, examining the deepeſt part of the earth, I found it exactly like the upper part, and of the ſame nature as the earth of all the country, as likewiſe of two pits, which I had dug at the diſtance of ſeventy or one hundred paces E. and W. of the maſs. Finding here no root or trace of generation, I reaſoned in the following manner.

Either this maſs was produced in the ſpot where it lies, or it was conveyed hither by human art, or caſt hither by ſome operation of nature. It could not be generated here, according to any known proceſs of nature. And whence, by whom, or how, could it be conveyed hither, as there are no iron mines within hundreds of leagues, nor remembrance that any have been worked in the kingdom? It could be of no value, ſince it could not be uſed; and why bring it into a country the moſt uninhabitable of all the Chaco, from the want of water? Beſides, how could ſo heavy a maſs be conveyed, the Indians never having known the uſe of wheel carriages? This maſs, therefore, muſt have been the effect of ſome volcanic exploſion.

Many circumſtances induce me to think ſo. Volcanos frequently leave behind them, after exploſion, pits of water, either hot or cold; and at the diſtance of about two leagues to the eaſt of this maſs, I diſcovered a brackiſh mineral ſpring, the only one to be found in all this country. In the whole diſtrict of the Chaco I travelled over, I obſerved no difference in the level of the ground, except the very ſpot where I made this diſcovery; and here only I found a gentle aſcent running from N. to S. and which attracted my notice before I had ſeen the maſs of iron. This aſcent is between four and ſix feet above the reſt of the country. The earth in every part around this maſs, as well as about the brackiſh ſpring, is a very light, looſe earth, like aſhes, even in colour. The graſs produced immediately contiguous, called Ahivi, is ſhort, ſmall, and extremely unpalatable to cattle; whereas the graſs found in the reſt of the country, at a little diſtance from the maſs and ſalt ſpring, is long and very grateful to them.

At a little depth in the earth are found ſtones of quartz, of a beautiful red colour, which the honey-gatherers make uſe of as flints to light their fires. They had formerly carried ſome of them away, on account of their peculiar beauty, being ſpotted and ſtudded as it were with gold. One of theſe, that weighed about an ounce, came into the hands of the Governor of Santiago del Eſtero, who told me, that he ground it, and ſhewed me more than a drachm of gold that he had extracted from it.

It is an undoubted fact, that in theſe immenſe foreſts there exiſts a maſs of pure iron, in the ſhape of a tree, with its branches. Many of the Indians have ſeen it; and the inhabitants of the colony of the Avipones, are acquainted with the ſpot where it lies. A diſtinguiſhed European of the city of Salta touched it. The body of this tree extends upon the ground in the direction from E. to W. having left behind to the E. a part which lies in the direction from N. to S. and it is from this that the pieces of metal may have been taken with a chiſſel. From this account one may venture to form the following hypotheſis.

I will ſuppoſe, that the volcanic exploſion happened in the ſpot where I diſcovered the brackiſh ſpring; that by the exploſion a great quantity of earth was raiſed, and formed the elevation of this part of the plain above the reſt; that it was originally much higher, but the continual rains of the Chaco, which is overflowed a third part of the year, are always acting to bring it to a level with the reſt of the country.

The direction of the greateſt portion of the maſs of iron ejected was from E. to W. and being heavier reached to the diſtance where it is found. Another portion of the matter ſmaller, and perhaps more fluid, ſeparated and took another direction, running into ſeveral ſtreams, as when water is thrown out of a pail. This ſmall portion of matter having cooled, and the earth which ſupported it being waſhed away gradually by the water, muſt, I apprehend, have formed what is now called the Tree of iron.

The ſaline and antimonial matters, which uſually accompany all minerals, muſt have been ſcattered round about in a ſimilar manner, and rendered the ground barren.

In the kingdom of Santa Fe de Bogotà, there is found duſt of platina mixed with gold. Almoſt every body knows the great affinity there is between theſe two metals, ſo that it is not ſurprizing, conſidering all theſe reaſons, that the fire of the volcano ſhould have melted the platina which lay above the gold, and thrown it up. This principle of volcanos is the moſt natural to account for the formation of thoſe famous maſſes of ſilver found ſeparate at Guantajaia, about which ſo many extravagant and ridiculous ſtories have been told.

The maſs of iron, which is the ſubject of this letter, according to its cubic meaſure, and allowing it a little more ſpecific gravity than iron, muſt weigh about 300 quintals.

(Sig.) Chev. MICH. RUBIN DE CELIS.

From the Iſland of Leon,
June 20, 1786.

A

METEOROLOGICAL JOURNAL

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METEOROLOGICAL JOURNAL

for January 1787.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	37	45½	30,42	0,042	SW	1	Fine.
	2	0	43½	47½	30,39		SSW	1	Cloudy.
2	8	0	34	45	30,42		WSW	1	Foggy.
	2	0	36	45	30,40		WSW	1	Cloudy.
3	8	0	37	47½	30,34	0,037	SW	1	Cloudy.
	2	0	40	50	30,37		WSW	1	Cloudy.
4	8	0	40	48	30,51		WSW	1	Cloudy.
	2	0	44	51	30,53		WSW	1	Cloudy.
5	8	0	42½	50	30,45		WSW	1	Cloudy.
	2	0	47	52	30,42		WNW	1	Cloudy.
6	8	0	44	51	30,35		NW	1	Cloudy.
	2	0	47	52	30,37		NNE	1	Cloudy.
7	8	0	33	50½	30,60		NE	1	Fine.
	2	0	36	51	30,62		ENE	1	Fine.
8	8	0	27	48	30,64		ESE	1	Foggy.
	2	0	34	50	30,63		NE by N	1	Cloudy.
9	8	0	36	49	30,63	0,017	NNE	1	Foggy.
	2	0	40	50½	30,60		NNE	1	Cloudy.
10	8	0	35	49	30,44		SE	1	Cloudy.
	2	0	34½	50½	30,41		SE	1	Cloudy.
11	8	0	34½	48½	30,19		SE	1	Cloudy.
	2	0	40	50½	30,10		SE by S	1	Fine.
12	8	0	34½	49	29,83		SE	1	Fine.
	2	0	40	52	29,68		SE by E	2	Fine.
13	8	0	37	48	29,52		SE by S	2	Fine.
	2	0	45	50	29,59		SSE	2	Cloudy.
14	8	0	37	48	29,89		SE by E	1	Foggy.
	2	0	45	50	29,91		SE by E	1	Fine.
15	8	0	33½	48	30,04		SE	1	Foggy.
	2	0	34	50½	30,02		ESE	1	Foggy.
16	8	0	30	48	29,90		SE by S	1	Foggy.
	2	0	36	49	29,85		SE	1	Cloudy.

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for January 1787.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	35½	47	30,07		NNE	1	Cloudy.
	2	0	40	49	30,16		NNE	1	Cloudy.
18	8	0	30	47	30,26		SW	1	Foggy.
	2	0	38	50	30,13		WSW	1	Rainy.
19	8	0	33	48	30,27	0,010	NW	1	Foggy.
	2	0	39½	50	30,25		NW	1	Foggy.
20	8	0	43	48	30,16	0,095	NW	1	Foggy.
	2	0	49	51	30,16		NNW	1	Cloudy.
21	8	0	44	49	30,16	0.018	NW	1	Foggy.
	2	0	47	52	30,12		NW by N	1	Cloudy.
22	8	0	43	49	30,12		NW	1	Cloudy.
	2	0	46	53	30,13		NNE	1	Cloudy.
23	8	0	41½	52	30,09	0,016	SW by S	1	Cloudy.
	2	0	43	53	30,08		W	1	Cloudy.
24	8	0	41	52	30,09		NNE	1	Foggy.
	2	0	44	54	30,10		E by N	1	Fine.
25	8	0	37	51½	30,15	0,029	ESE	1	Cloudy.
	2	0	38	53	30,18		SE by S	1	Fine.
26	8	0	28½	51	30,15		NE by E	1	Fine.
	2	0	33½	52	30,05		NE	2	Cloudy.
27	8	0	28	49½	29,84		NE	1	Snow.
	2	0	31	50	29,83		NE by E	1	Cloudy.
28	8	0	30	47½	29,67		SE by S	3	Cloudy.
	2	0	31½	49	29,64		SSE	2	Snow.
29	8	0	40½	47½	29,88	0,068	S by E	2	Cloudy.
	2	0	45	49	29,93		SSW	2	Cloudy.
30	8	0	47	49	30,27	0,028	SSW	2	Cloudy.
	2	0	51	51½	30,32		SSW	2	Fine.
31	8	0	38	50	30,38		SW	1	Fine.
	2	0	48½	53	30,38		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	41	50	30,31		SW	1	Fine.
	2	0	50	54	30,25		SW	1	Fine.
2	8	0	44	52	30,28	0,034	SE by E	1	Foggy.
	2	0	48	54	30,28		SSW	1	Cloudy.
3	8	0	45	53	30,11		SW	1	Cloudy.
	2	0	49	54½	30,01		SW	2	Cloudy.
4	8	0	35	53	30,25	0,046	SE by E	1	Fine.
	2	0	44	54	30,21		SE	1	Fine.
5	8	0	33	52½	30,04		SE by E	1	Cloudy.
	2	0	41	53	29,95		SE by E	1	Fine.
6	8	0	37	51	29,87		SW by S	1	Cloudy.
	2	0	44	53	29,82		SW	1	Cloudy.
7	8	0	48	52	29,55	0,020	S by W	2	Rainy.
	2	0	52	54	29,55		SSW	2	Cloudy.
8	8	0	37	52	30,01	0,018	SW	1	Fine.
	2	0	49	54	30,01		SW by W	2	Cloudy.
9	8	0	46½	53	29,77		SW by S	2	Rainy.
	2	0	50	54	29,61		SW by W	2	Cloudy.
10	8	0	43½	53	29,41	0,058	S by E	2	Cloudy.
	2	0	47	54	29,24		SSE	2	Rainy.
11	8	0	41	53	29,21	0,160	S by W	2	Fair.
	2	0	45	54	29,05		S by W	2	Rainy.
12	8	0	39½	53	28,68	0,206	SW by S	3	Rainy.
	2	0	45	53	28,67		SSW	3	Rainy.
13	8	0	41	52	28,71	0,376	SW	1	Cloudy.
	2	0	45½	53	28,99		W	1	Cloudy.
14	8	0	35	51	29,71		W by N	1	Fine.
	2	0	45	54	29,86		W by N	1	Fine.
15	8	0	46	52	29,83		SW by W	2	Cloudy.
	2	0	49	51	29,81		SW	2	Cloudy.
16	8	0	49	53	29,96	0,026	SW	2	Cloudy.
	2	0	51	55	29,85		SW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	38	54	30,31		SW	1	Fine.
	2	0	47	56	30,36		W	1	Fine.
18	8	0	42	54	30,30		SW by S	1	Cloudy.
	2	0	50	55	30,27		W	1	Cloudy.
19	8	0	39	54	30,34		SW	1	Fine.
	2	0	52	56	30,33		SW by W	1	Fine.
20	8	0	40	54	30,27		S by E	1	Foggy.
	2	0	45	55	30,27		SE by S	1	Foggy.
21	8	0	41½	54	30,18		N	1	Fine.
	2	0	46	55	30,16		NNE	1	Cloudy.
22	8	0	33½	53	30,16		ENE	1	Fine.
	2	0	41	55	30,16		E	1	Fine.
23	8	0	31	52	30,14		ESE	1	Foggy.
	2	0	41	55	30,14		ESE	1	Fine.
24	8	0	36	52	30,08		W	1	Cloudy.
	2	0	44	53	30,06		W	1	Cloudy.
25	8	0	38	52	30,08		W	1	Cloudy.
	2	0	46	53	30,03		W	1	Cloudy.
26	8	0	42	52	30,06	0,024	W by N	1	Cloudy.
	2	0	51	53	30,08		W by S	1	Cloudy.
27	8	0	40	51	30,11		SW by S	1	Fine.
	2	0	49	53	30,00		SW	1	Fine.
28	8	0	43	51	29,90	0,073	W	1	Fine.
	2	0	53	54	29,97		W by S	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M			Inches	Inch.	Points.	Str.	
March 1	7	0	51	53	29,80	0,107	SW	2	Cloudy.
	2	0	55	55	29,62		SW	2	Rainy.
2	7	0	49	54	29,32	0,036	SW by S	3	Cloudy.
	2	0	53	54	29,38		SW by S	2	Cloudy.
3	7	0	46	53½	29,45		SW by S	2	Cloudy.
	2	0	49	54	29,03		SSW	2	Rainy.
4	7	0	42	53	29,10	0,076	W	1	Fair.
	2	0	45	54	29,55		NNW	1	Cloudy.
5	7	0	36	52	29,75		SE	1	Cloudy.
	2	0	37	53	29,55		SE by E	1	Rainy.
6	7	0	44	52	29,44	0,061	SSE	2	Cloudy.
	2	0	47	54	29,11		S by E	2	Rainy.
7	7	0	40	53	29,31	0,261	WNW	1	Fair.
	2	0	49	55	29,52		WNW	1	Fine.
8	7	0	34	53	29,88		WNW	1	Fine.
	2	0	47½	54½	29,92		NW	1	Fine.
9	7	0	38	53	29,53		SE	2	Hazy.
	2	0	45	54	29,45		SSE	2	Cloudy.
10	7	0	40	53	29,17		S	2	Rainy.
	2	0	49	55	28,93		SW	2	Rainy.
11	7	0	40	52½	29,85	0,278	W	1	Fine.
	2	0	46½	54½	30,01		W by S	1	Cloudy.
12	7	0	35	51½	30,37	0,022	W	1	Foggy.
	2	0	47	54½	30,41		W	1	Fine.
13	7	0	41½	52	30,51		W by S	1	Cloudy.
	2	0	52	54½	30,51		WSW	1	Cloudy.
14	7	0	45	53½	30,51		WSW	1	Cloudy.
	2	0	49	55	30,51		NW	1	Cloudy.
15	7	0	41½	53	30,42		W	1	Fine.
	2	0	55	55	30,40		W	1	Cloudy.
16	7	0	41	54	30,38		W	1	Fine.
	2	0	52	55	30,33		W	1	Cloudy.

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for March 1787.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
March 17	7	0	46	53	30,28		WNW	1	Fine.
	2	0	51	55	30,29		NWbyW	1	Cloudy.
18	7	0	43	53	30,50		NNW	1	Fine.
	2	0	52	50	30,49		NNW	1	Fine.
19	7	0	38	52	30,41		W	1	Fine.
	2	0	53	55	30,31		W by S	1	Fine.
20	7	0	40	53	30,24		SW	1	Foggy.
	2	0	54	56	30,22		S by W	1	Fine.
21	7	0	42	55	30,23		SW	1	Foggy.
	2	0	56	57	30,21		W	1	Fine.
22	7	0	42	55	30,11		SSW	1	Fine.
	2	0	56	56	30,02		SSW	1	Hazy.
23	7	0	46	55	29,48	0,019	S	1	Rainy.
	2	0	54	56	29,44		SSW	1	Fine.
24	7	0	38	54	29,51		NW	1	Fine.
	2	0	51	57	29,60		NNW	1	Fine.
25	7	0	42	55	29,75		SW	2	Fine.
	2	0	47	56	29,63		S	2	Rainy.
26	7	0	48	54	29 55	0,262	S by W	2	Cloudy.
	2	0	53	57	29,58		S by W	2	Cloudy.
27	7	0	44	54	29,70		SW	2	Fine.
	2	0	53	57	29,70		SW by W	2	Fine.
28	7	0	47	55	29,58		WSW	2	Cloudy.
	2	0	56	57	29,51		SW	2	Cloudy.
29	7	0	48	55	29,30	0,016	SW	1	Cloudy.
	2	0	53	56	29,28		W by S	1	Cloudy.
30	7	0	48	56	29,16	0,120	WSW	1	Cloudy.
	2	0	52	57	29,26		SW by S	1	Cloudy.
31	7	0	42	56	29,51	0,025	SSW	1	Fine.
	2	0	52	58	29,59		SSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
April 1	7	0	45	56	29,98		ENE	1	Cloudy.
	2	0	49	57	30,02		NE	1	Cloudy.
2	7	0	44	54	29,96		NE	1	Cloudy.
	2	0	48	55	29,88		ENE	2	Cloudy.
3	7	0	45	54	29,68	0,117	ENE	1	Cloudy.
	2	0	49	55	29,66		E	1	Rainy.
4	7	0	44	54	29,74	0,049	NNE	1	Cloudy.
	2	0	53	57	29,78		ENE	1	Fine.
5	7	0	45	55	29,84		NE	1	Cloudy.
	2	0	52	56	29,82		NE	1	Cloudy.
6	7	0	48	55	29,76		SSE	1	Rainy.
	2	0	51	57	29,72		SE	1	Cloudy.
7	7	0	45	55	29,77	0,021	NE	2	Cloudy.
	2	0	49	57	29,85		E by N	2	Fine.
8	7	0	42	55	30,08		ENE	2	Cloudy.
	2	0	47	56	30,12		NE by E	2	Cloudy.
9	7	0	38	53	30,28		ENE	2	Fine.
	2	0	52	55	30,24		ENE	2	Fine.
10	7	0	40	53	30,24		ENE	1	Fine.
	2	0	56	57	30,16		NE by E	1	Fine.
11	7	0	43	55	30,07	0,027	SE by E	1	Cloudy.
	2	0	52	56	30,07		E	1	Hazy.
12	7	0	40	54	30,07		NE	1	Cloudy.
	2	0	54	57	30,02		NE by E	1	Fine.
13	7	0	45	55	29,92	0,034	N	1	Cloudy.
	2	0	47	56	29,88		NNW	2	Cloudy.
14	7	0	37	55	29,84		NNE	2	Hazy.
	2	0	48	55	29,78		NNE	2	Cloudy.
15	7	0	40	55	29,80		S	1	Fine.
	2	0	53	57	29,88		S	2	Cloudy.
16	7	0	45	54	30,09		NE	1	Cloudy.
	2	0	53	57	30,13		NE by E	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
April 17	7	0	40	54	30,40		NE	1	Cloudy.
	2	0	43	54	30,40		NE	1	Cloudy.
18	7	0	45	54	30,13		N	2	Cloudy.
	2	0	51	55	30,00		NW	2	Cloudy.
19	7	0	45	54	30,08	0,023	NNW	2	Fine.
	2	0	52	54	30,13		N	2	Cloudy.
20	7	0	42	53	30,34		NE	2	Cloudy.
	2	0	46	55	30,38		NE	2	Cloudy.
21	7	0	41	53	30,48		NE	2	Cloudy.
	2	0	51	55	30,42		NE	2	Cloudy.
22	7	0	44	53	30,27		NW	2	Cloudy.
	2	0	57	55	30 22		W by N	2	Cloudy.
23	7	0	48	53	30,20		W	2	Cloudy.
	2	0	58	56	30,18		W	2	Cloudy.
24	7	0	47	54	30,20		NE	1	Cloudy.
	2	0	56	57	30,22		NE	1	Cloudy.
25	7	0	48	55	29,88		W by S	1	Cloudy.
	2	0	50	56	29,56		SW	1	Rainy.
26	7	0	47	55	29,57	0,197	NNW	2	Fine.
	2	0	55	57	29,68		NW	2	Cloudy.
27	7	0	47	56	29,77		WSW	1	Fine.
	2	0	54	58	29,57		SW	1	Rainy.
28	7	0	46	56	29,07	0,170	WSW	2	Fair.
	2	0	49	57	29,17		W	2	Cloudy.
29	7	0	46	55	29,27	0,052	SW	2	Cloudy.
	2	0	49	57	29,19		W by N	2	Cloudy.
30	7	0	42	54	29,32	0,233	NW	2	Cloudy.
	2	0	45	55	29,48		N	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May	7	0	39	53	29,82		NNE	2	Fine.
	2	0	50	55	29,87		N	2	Cloudy.
2	7	0	42	53	29,97		SSE	1	Cloudy.
	2	0	58	55	29,92		W by S	1	Fine.
3	7	0	50	55	29,93		W	1	Fine.
	2	0	60	57	29,88		WSW	1	Fine.
4	7	0	55	55	29,82		WSW	1	Cloudy.
	2	0	62	59	29,91			0	Cloudy.
5	7	0	46	57	30,08		NNW	2	Fine.
	2	0	54	59	30,21		NNE	2	Fine.
6	7	0	41	56	30,30		NNE	1	Fine.
	2	0	51	57	30,28		NE	2	Cloudy.
7	7	0	43	56	30,28		NE by E	1	Fine.
	2	0	57	59	30,28		NE by E	1	Fine.
8	7	0	48	57	30,16		NE	1	Fine.
	2	0	62	59	30,05		NE	1	Fine.
9	7	0	49	57	29,84		NNE	1	Fine.
	2	0	60	60	29,76		NE	1	Hazy.
10	7	0	46	58	29,66		NE	1	Cloudy.
	2	0	49	60	29,54		N	1	Rainy.
11	7	0	47	58	29,45	0,631	SW	1	Cloudy.
	2	0	55	59	29,55		SW	1	Cloudy.
12	7	0	44	58	29,76		W	1	Hazy.
	2	0	61	60	29,76		W	1	Cloudy.
13	7	0	50	59	29,74		WSW	1	Fine.
	2	0	64	60	29,70		W by S	1	Fine.
14	7	0	52	59	29,80		E by N	1	Fine.
	2	0	63	61	29,85		E by N	1	Cloudy.
15	7	0	50	60	30,07		E	1	Fine.
	2	0	56	60	30,09		ESE	1	Cloudy.
16	7	0	48	60	30,12		SE	2	Cloudy.
	2	0	54	60	30,11		SE	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 17	7	0	49	60	30,02		SE	2	Fine.
	2	0	56	61	30,02		SE	2	Fine.
18	7	0	49	60	30,11		ESE	1	Fine.
	2	0	60	61	30,15		SE by E	1	Fine.
19	7	0	52	60	30,25		SSE	1	Fine.
	2	0	65	62	30,26		SSE	1	Fine.
20	7	0	52	61	30,26		SSE	1	Fine.
	2	0	66	62	30,26		SE	2	Fine.
21	7	0	57	62	30,18		SE	1	Cloudy.
	2	0	70	63	30,14		SE	1	Fine.
22	7	0	56	62	30,08		W	1	Fine.
	2	0	71	64	30,06		NW	1	Fine.
23	7	0	53	63	29,97		W	1	Cloudy.
	2	0	58	63	29,85		W	1	Cloudy.
24	7	0	49	61	29,56	0,137	W	2	Rainy.
	2	0	55	63	29,33		W	2	Fair.
25	7	0	50	62	29,50	0,100	WNW	1	Cloudy.
	2	0	61	63	29,63		WNW	1	Fine.
26	7	0	53	62	29,53	0,060	WNW	2	Fine.
	2	0	64	62	29,49		W	2	Cloudy.
27	7	0	52	61	29,56	0,130	W by N	1	Cloudy.
	2	0	62	62	29,58		W by N	1	Fine.
28	7	0	52	61	29,45	0,059	W	2	Cloudy.
	2	0	59	62	29,46		WNW	2	Cloudy.
29	7	0	49	60	29,77	0,020	WNW	2	Cloudy.
	2	0	57	62	29,83		WNW	2	Cloudy.
30	7	0	50	59	29,93		NNE	1	Fine.
	2	0	61	61	30,08		NNE	1	Fine.
31	7	0	52	60	30,31		NE	1	Fine.
	2	0	69	61	30,29		NNE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	57	60	30,21		SE	1	Fine.
	2	0	65	62	30,10		N	1	Cloudy.
2	7	0	51	61	30,00		NNE	1	Cloudy.
	2	0	55	61	30,01		NNE	1	Cloudy.
3	7	0	51	61	29,96		SSW	1	Fine.
	2	0	65	62	29,90		NW	1	Fine.
4	7	0	51	60	29,96		NE	1	Cloudy.
	2	0	62	61	29,91		NW	1	Cloudy.
5	7	0	52	61	29,67		SSW	1	Fine.
	2	0	64	62	29,64		WSW	1	Cloudy.
6	7	0	49	60	29,77		NE	1	Cloudy.
	2	0	50	61	29,81		NE by E	1	Cloudy.
7	7	0	46	59	30,00	0,076	SW	1	Fine.
	2	0	60	60	30,00		SW	1	Fine.
8	7	0	51	59	30 14		SSE	1	Fine.
	2	0	63	60	30,13		SE by S	1	Fine.
9	7	0	59	60	30,18		SE by S	1	Fine.
	2	0	69	62	30,16		SE by S	1	Fine.
10	7	0	58	60	30,14		SE by E	1	Fine.
	2	0	68	63	30,11		SE	1	Fine.
11	7	0	58	63	30,05		E by S	1	Fine.
	2	0	73	64	30,04		E by S	1	Fine.
12	7	0	58	63	30,04		E	1	Fine.
	2	0	77	66	30,00		E	1	Fine.
13	7	0	58	64	29,97		ENE	1	Cloudy.
	2	0	64	65	29,90		ENE	1	Cloudy.
14	7	0	55	65	29,70	0,175	E	1	Rainy.
	2	0	63	65	29,68		SSW	2	Cloudy.
15	7	0	58	65	29,68		SSE	1	Fine.
	2	0	73	65	29,68		SSE	1	Hazy.
16	7	0	59	65	29,90		S by E	1	Fine.
	2	0	73	67	29,94		S	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	58	66	30,04		SW	1	Fine.
	2	0	67	66	30,05		SW	1	Cloudy.
18	7	0	58	65	30,08		SW	1	Fine.
	2	0	66	65	30,00		SW	1	Cloudy.
19	7	0	55	65	29,83	0,065	WSW	1	Cloudy.
	2	0	64	65	29,81		SW by W	1	Cloudy.
20	7	0	54	63	29,80		WSW	1	Cloudy.
	2	0	67	65	29,75		SW	1	Cloudy.
21	7	0	55	64	29,76		SE	1	Hazy.
	2	0	62	64	29,74		SE	1	Cloudy.
22	7	0	58	64	29,70	0,020	SSW	1	Cloudy.
	2	0	68	65	29,74		SW	1	Fine.
23	7	0	60	65	29,79	0,017	SW by W	1	Cloudy.
	2	0	71	66	29,87		WSW	1	Cloudy.
24	7	0	58	65	30,01		SSE	1	Fine.
	2	0	71	67	29,94		S by W	1	Fine.
25	7	0	59	66	29,71	0,209	SW	2	Cloudy.
	2	0	67	66	29,72		SW	2	Cloudy.
26	7	0	57	65	29,78		SW by S	1	Cloudy.
	2	0	62	66	29,78		SSW	2	Cloudy.
27	7	0	58	66	29,55	0,100	SSW	2	Cloudy.
	2	0	67	66	29,55		S by W	2	Cloudy.
28	7	0	56	65	29,82		SW	1	Hazy.
	2	0	65	66	29,82		SW by S	1	Cloudy.
29	7	0	55	65	29,88		SSW	1	Fine.
	2	0	68	66	29,88		SSW	1	Hazy.
30	7	0	56	65	29,98		SW	1	Hazy.
	2	0	72	67	30,03		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	61	65	30,22		SE by E	1	Fine.
	2	0	73	67	30,26		ESE	1	Cloudy.
2	7	0	59	66	30,38		ESE	1	Fine.
	2	0	77	68	30,33		ESE	1	Fine.
3	7	0	63	67	30,38		N	1	Fine.
	2	0	78	69	30,35		NNE	1	Fine.
4	7	0	66	69	30,37		NE	1	Fine.
	2	0	77	71	30,37		NE	1	Fine.
5	7	0	65	70	30,24		ESE	1	Fine.
	2	0	78	72	30,18		ESE	1	Fine.
6	7	0	62	70	29,94		SE	1	Fine.
	2	0	77	73	29,80		S	1	Fine.
7	7	0	60	70	29,70		SW	1	Cloudy.
	2	0	71	70	29,70		W	1	Fine.
8	7	0	58	69	29,70		WSW	1	Fine.
	2	0	71	70	29,76		WSW	1	Fine.
9	7	0	59	68	29,95		WSW	1	Fine.
	2	0	65	67	29,94		WSW	1	Cloudy.
10	7	0	57	67	29,73	0,131	SSW	1	Cloudy.
	2	0	68	68	29,74		SW	1	Cloudy.
11	7	0	55	65	29,98	0,083	SW	1	Fine.
	2	0	67	69	29,97		SW	1	Fine.
12	7	0	58	65	29,73	0,190	WSW	1	Cloudy.
	2	0	67	66	29,69		WSW	1	Cloudy.
13	7	0	58	66	29,66	0,412	SW	1	Cloudy.
	2	0	58	65	29,61		SSW	1	Rainy.
14	7	0	57	65	29,55	0,230	NNE	1	Fair.
	2	0	65	65	29,59		NE	1	Cloudy.
15	7	0	56	65	29,75	0,045	SSE	1	Cloudy.
	2	0	66	66	29,71		S	1	Cloudy.
16	7	0	57	66	29,60	0,244	SSW	2	Fine.
	2	0	65	66	29,74		SSW	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	55	65	29 73	0,094	S by W	1	Rainy.
	2	0	63	65	29,73		SSW	2	Rainy.
18	7	0	54	64	29 94	0,183	SW by W	1	Fine.
	2	0	62	64	29 93		SW	1	Fine.
19	7	0	51	63	30,04	0,044	WSW	1	Fine.
	2	0	66	64	30,04		W	1	Cloudy.
20	7	0	58	63	29,88		SW by S	1	Cloudy.
	2	0	62	64	29,78		SW by S	1	Cloudy.
21	7	0	57	63	29,52	0,015	SW by S	1	Cloudy.
	2	0	60	64	29,47		SW by S	1	Rainy.
22	7	0	56	63	29,44	0,220	SW by W	1	Cloudy.
	2	0	63	64	29,44		SW by W	1	Fine.
23	7	0	58	63	29,49	0,173	SSW	1	Cloudy.
	2	0	63	64	29 49		SSW	1	Rainy.
24	7	0	56	64	29,37	0,305	SW by S	1	Cloudy.
	2	0	60	64	29,40		SW by S	1	Rainy.
25	7	0	57	63	29,54	0,195	SW by S	1	Cloudy.
	2	0	63	64	29,51		SW	1	Cloudy.
26	7	0	55	63	29,71	0,235	SW by W	1	Fine.
	2	0	64	64	29,75		SW by W	1	Fine.
27	7	0	56	63	29,92	0,111	WSW	1	Fine.
	2	0	66	64	29,94		SW	1	Fine.
28	7	0	57	64	30,00	0,161	S	1	Fine.
	2	0	67	65	29,95		S	1	Hazy.
29	7	0	62	65	29,64		SW	1	Cloudy.
	2	0	64	65	29,66		SW	1	Cloudy.
30	7	0	62	65	29,71	0,053	SW by S.	2	Cloudy.
	2	0	67	66	29,70		SW	2	Fine.
31	7	0	54	65	29,80	0,122	SW by S	1	Rainy.
	2	0	66	66	29,77		SW by S	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Auguſt 1	7	0	57	65	29,94	0,425	SW	2	Cloudy.
	2	0	68	66	29,94		SW	2	Fine.
2	7	0	56	65	30,18		WSW	1	Fine.
	2	0	67	67	30,26		WSW	1	Fine.
3	7	0	57	67	30,40		WSW	1	Fine.
	2	0	75	68	30,40		W	1	Fine.
4	7	0	60	67	30,38		WSW	1	Fine.
	2	0	77	68	30,36		WSW	1	Fine.
5	7	0	64	68	30,18		ESE	1	Hazy.
	2	0	78	68	29,98		SE	1	Fine.
6	7	0	62	68	30,02		SW	1	Cloudy.
	2	0	75	69	30,04		WSW	1	Cloudy.
7	7	0	61	70	30,16		ESE	1	Fine.
	2	0	77	71	30,16		ESE	1	Fine.
8	7	0	63	70	30,22		SE	1	Fine.
	2	0	77	72	30,18		SE	1	Fine.
9	7	0	63	71	30,06		ESE	1	Fine.
	2	0	83	73	29,99		E by S	1	Fine.
10	7	0	64	72	29,91		WSW	1	Cloudy.
	2	0	77	73	30,00		WSW	1	Cloudy.
11	7	0	63	71	30,04	0,060	WSW	1	Cloudy.
	2	0	74	71	30,05		WSW	1	Fine.
12	7	0	58	70	30,86	0,036	SW	1	Cloudy.
	2	0	67	70	29,86		SW	1	Cloudy.
13	7	0	55	68	30,23	0,044	SW	1	Cloudy.
	2	0	72	69	30,22		SW	1	Fine.
14	7	0	55	68	30,13		WSW	1	Cloudy.
	2	0	67	68	30,03		WSW	2	Cloudy.
15	7	0	62	68	29,90	0,075	SW by W	2	Rainy.
	2	0	68	68	29,90		SW by W	1	Fair.
16	7	0	63	68	29,90		SW	1	Cloudy.
	2	0	67	68	29,87		SW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain .	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Auguſt 17	7	0	57	68	29,70		SW by W	2	Cloudy.
	2	0	68	68	29,73		SW by W	2	Cloudy.
18	7	0	54	66	29,81	0,068	SW by S	2	Fine.
	2	0	65	66	29,76		SSW	2	Hazy.
19	7	0	54	66	29,72	0,060	SSW	1	Fine.
	2	0	67	66	29,70		SSW	1	Cloudy.
20	7	0	57	65	29,92		W	1	Cloudy.
	2	0	68	66	29,96		W	1	Fine.
21	7	0	54	66	30,18		WNW	1	Fine.
	2	0	69	66	30,21		W	1	Cloudy.
22	7	0	54	66	30,20		W	1	Fine.
	2	0	70	66	30,10		WSW	1	Fine.
23	7	0	59	67	29,89	0,059	WSW	1	Cloudy.
	2	0	70	67	29,89		WSW	1	Cloudy.
24	7	0	52	65	29,87		WSW	1	Cloudy.
	2	0	58	66	29,70		WSW	1	Cloudy.
25	7	0	52	65	29,28	0,107	WSW	1	Cloudy.
	2	0	54	66	29,25		WSW	2	Rainy.
26	7	0	48	62	29,68	0,220	WSW	2	Fine.
	2	0	63	63	29,72		WSW	2	Cloudy.
27	7	0	54.	63	29,85	0,017	NNW	2	Cloudy.
	2	0	63	63	29,92		NNW	1	Cloudy.
28	7	0	54	63	30,05		NNW	1	Cloudy.
	2	0	63	63	30,05		NNW	1	Cloudy.
29	7	0	54	62	30,05		WNW	1	Cloudy.
	2	0	68	63	30,05		WNW	1	Cloudy.
30	7	0	58	63	30,15		WNW	1	Cloudy.
	2	0	65	63	30,23		WNW	1	Cloudy.
31	7	0	58	63	30,34		NW	1	Cloudy.
	2	0	67	64	30,35		NNW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	58	63	30,38		NNE	1	Cloudy.
	2	0	62	63	30,38		NNE	1	Cloudy.
2	7	0	55	63	30,40		NE	1	Cloudy.
	2	0	63	63	30,40		NE	1	Cloudy.
3	7	0	54	62	30,44		ENE	1	Cloudy.
	2	0	61	63	30,45		ENE	1	Cloudy.
4	7	0	57	62	30,40		ENE	1	Cloudy.
	2	0	64	63	30,36		ENE	1	Cloudy.
5	7	0	60	62	30,29		ESE	1	Cloudy.
	2	0	68	63	30,24		ESE	2	Cloudy.
6	7	0	58	63	30,11		ESE	1	Cloudy.
	2	0	65	64	30,05		ESE	1	Cloudy.
7	7	0	57	63	30,02		SE	1	Cloudy.
	2	0	65	64	30,02		SE	1	Cloudy.
8	7	0	56	63	30,13		SE	1	Cloudy.
	2	0	66	64	30,03		SE	1	Cloudy.
9	7	0	56	63	30,18		SE	1	Cloudy.
	2	0	64	64	30,18		SE	1	Cloudy.
10	7	0	52	62	30,23		SSE	1	Fine.
	2	0	62	63	30,22		SE	1	Cloudy.
11	7	0	53	62	30,28		SSE	1	Cloudy.
	2	0	63	63	30,28		SE	1	Cloudy.
12	7	0	53	61	30,30		ESE	1	Cloudy.
	2	0	61	62	30,28		E	1	Cloudy.
13	7	0	53	61	30,23		ESE	1	Cloudy.
	2	0	61	62	30,18		SE	1	Cloudy.
14	7	0	53	61	30,10		NE	1	Cloudy.
	2	0	62	61	30,05		ENE	1	Cloudy.
15	7	0	54	61	29,93		SSE	1	Cloudy.
	2	0	62	62	29,86		SSE	1	Cloudy.
16	7	0	57	61	29,46	0,160	SSE	1	Rainy.
	2	0	64	62	29,30		SSW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	52	61	29,26	0,151	SW	1	Fine.
	2	0	58	62	29,11		SW	1	Rainy.
18	7	0	53	62	29,18	0,135	WSW	1	Cloudy.
	2	0	62	62	29,29		WSW	1	Fine.
19	7	0	51	61	29,47	0,162	WSW	1	Fine.
	2	0	58	62	29,45		WSW	1	Rainy.
20	7	0	47	60	29,78	0,066	WSW	1	Fine.
	2	0	63	62	29,78		WSW	2	Cloudy.
21	7	0	58	61	29,51	0,155	SSE	2	Cloudy.
	2	0	63	62	29,54		SSE	2	Cloudy.
22	7	0	56	62	29,68	0,046	SE by S	2	Rainy.
	2	0	65	62	29,61		S	2	Fair.
23	7	0	55	62	29,85	0,017	SSW	2	Cloudy.
	2	0	65	63	29,86		SSW	2	Fine.
24	7	0	57	62	29,92		SSW	1	Cloudy.
	2	0	65	63	29.94		SSW	2	Cloudy.
25	7	0	53	63	29,88	0,098	SSE	1	Foggy.
	2	0	65	64	29,77		SE	1	Cloudy.
26	7	0	59	63	29,68		ESE	1	Cloudy.
	2	0	68	64	29,70		SSE	2	Fine.
27	7	0	56	64	29,76		S	1	Foggy.
	2	0	57	63	29,80		S	1	Cloudy.
28	7	0	48	62	29,98		S	1	Cloudy.
	2	0	57	62	30,00		S	1	Cloudy.
29	7	0	46	61	30,00		S	1	Fine.
	2	0	59	61	29,95		SE	1	Cloudy.
30	7	0	49	61	29,83		ESE	1	Cloudy.
	2	0	59	61	29,72		SE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	54	61	29,56		SE	1	Cloudy.
	2	0	63	62	29,55		SE	1	Cloudy.
2	7	0	58	61	29,71		E	1	Cloudy.
	2	0	63	62	29,75		ESE	1	Fine.
3	7	0	56	61	29,65		S	1	Cloudy.
	2	0	57	61	29,60		S	1	Small rain.
4	7	0	56	61	29,56		SSW	2	Cloudy.
	2	0	57	61	29,56		SSW	2	Cloudy.
5	7	0	51	61	29,78	0,125	SSW	2	Fine.
	2	0	58	60	29,82		SSW	2	Cloudy.
6	7	0	54	59	29,96	0,076	SSE	1	Cloudy.
	2	0	62	60	29,99		SSE	1	Cloudy.
7	7	0	54	60	30,04	0,212	ESE	1	Cloudy.
	2	0	62	61	30,08		ESE	1	Cloudy.
8	7	0	56	60	30,10		SE	1	Cloudy.
	2	0	61	61	30,09		SE	1	Cloudy.
9	7	0	47	60	30,09		SSE	1	Foggy.
	2	0	62	61	30,03		S	1	Cloudy.
10	7	0	56	60	29,53		SSW	1	Cloudy.
	2	0	56	60	29,27		SSW	2	Cloudy.
11	7	0	44	58	29,28	0,170	SSW	1	Cloudy.
	2	0	50	58	29,25		SSW	1	Cloudy.
12	7	0	39	56	29,36		SSW	2	Cloudy.
	2	0	51	57	29,35		SSW	2	Fine.
13	7	0	41	56	29,21		SW by S	1	Fine.
	2	0	56	57	29,24		SW	1	Fine.
14	7	0	41	56	29,42		SW by S	1	Foggy.
	2	0	55	57	29,57		SW by S	1	Hazy.
15	7	0	39	55	29,78		SW	1	Fine.
	2	0	56	57	29,77		SW	1	Fine.
16	7	0	55	56	29,55	0,315	WSW	1	Cloudy.
	2	0	60	58	29,66		WSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	55	57	29,74	0,015	SW by W	1	Cloudy.
	2	0	56	56	29,72		SW by W	1	Cloudy.
18	7	0	46	56	29,64	0,105	SW by W	1	Foggy.
	2	0	55	57	29,67		SW by W	1	Fine.
19	7	0	42	56	29,72		SW by W	1	Fine.
	2	0	52	57	29,78		WSW	1	Fine.
20	7	0	45	56	29,98		WSW	1	Fine.
	2	0	53	57	30,05		WSW	1	Cloudy.
21	7	0	45	55	30,15		WNW	1	Cloudy.
	2	0	48	57	30,13		NNW	1	Cloudy.
22	7	0	48	54	29,67	0,038	SSW	2	Rainy.
	2	0	56	56	29,57		SSW	2	Fair.
23	7	0	44	55	29,68		SW	1	Fine.
	2	0	54	57	29,77		WSW	1	Cloudy.
24	7	0	41	55	29,90		SW	1	Fine.
	2	0	52	56	29,72		SSW	1	Rainy.
25	7	0	44	55	29,79	0,223	SSW	1	Cloudy.
	2	0	53	57	29,47		S	2	Rainy.
26	7	0	47	56	29,60	0,284	WSW	1	Cloudy.
	2	0	52	57	29,70		WSW	1	Cloudy.
27	7	0	57	56	29,61	0,090	SW by S	1	Cloudy.
	2	0	59	60	29,72		SW by S	1	Cloudy.
28	7	0	58	58	29,90	0,130	S by W	1	Cloudy.
	2	0	60	58	29,94		SSW	1	Cloudy.
29	7	0	53	58	30,03		SW	1	Cloudy.
	2	0	58	60	30,07		SW	1	Rainy.
30	7	0	56	59	29,87	0,035	SW by S	1	Cloudy.
	2	0	60	61	29,78		SW	1	Cloudy.
31	7	0	53	60	29,55		SSW	1	Fine.
	2	0	58	62	29,42		SSW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	49	61	29,39	0,020	SW	1	Hazy.
	2	0	56	61	29,38		SW	1	Fine.
2	7	0	45	59	29,38	0,195	SSE	1	Foggy.
	2	0	50	61	29,27		SSE	1	Cloudy.
3	7	0	40	58	29,55	0,268	NNE	2	Fine.
	2	0	48	59	29,83		NNE	2	Fine.
4	7	0	36	56	29,96		SSE	1	Fine.
	2	0	48	56	29,67		S	2	Cloudy.
5	7	0	42	55	29,58	0,135	SSE	2	Cloudy.
	2	0	48	57	29,60		SSW	2	Fine.
6	7	0	48	57	29,55		WSW	2	Cloudy.
	2	0	52	59	29,55		WSW	2	Fine.
7	7	0	43	56	29,61		WSW	1	Fine.
	2	0	49	58	29,70		WSW	1	Fine.
8	7	0	47	56	29,54		SSE	1	Cloudy.
	2	0	52	58	29,18		S	2	Rainy.
9	7	0	47	56	29,12	0,305	SW by W	2	Fine.
	2	0	53	58	29,48		W	2	Fine.
10	7	0	48	58	29,74		S by E	1	Fine.
	2	0	54	60	29,71		S	1	Cloudy.
11	7	0	51	58	29,45		SSE	1	Cloudy.
	2	0	58	60	29,33		SE by S	1	Fine.
12	7	0	49	57	29,42	0,080	S	2	Rainy.
	2	0	53	58	29,48		SSE	1	Cloudy.
13	7	0	43	58	29,48		ESE	1	Foggy.
	2	0	54	59	29,60		E	2	Cloudy.
14	7	0	42	57	29,80		N	1	Fine.
	2	0	43	58	29,80		NNW	1	Fine.
15	7	0	38	56	29,97		N	1	Cloudy.
	2	0	43	57	30,01		N	1	Fine.
16	7	0	36	55	30,10		NNE	1	Fine.
	2	0	43	57	30,16		NNW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	7	0	41	55	30,10		NNW	1	Cloudy.
	2	0	44	56	29,98		NNW	1	Cloudy.
18	7	0	36	54	29,84		NW	1	Cloudy.
	2	0	40	53	29,85		NW	1	Fine.
19	7	0	33	51	29,86		NWbyW	1	Cloudy.
	2	0	40	53	29,62		W	1	Rainy.
20	7	0	34	51	29,68	0,337	NNW	1	Fine.
	2	0	42	53	29,75		NNW	1	Fine.
21	7	0	32	50	30,00		N	1	Hazy.
	2	0	40	52	30,00		NW	1	Cloudy.
22	7	0	42	51	29,88		W	1	Cloudy.
	2	0	44	54	29,74		WSW	1	Cloudy.
23	7	0	35	52	29,71		W	1	Fine.
	2	0	42	54	29,73		WSW	1	Cloudy.
24	7	0	33	52	29,82		WSW	1	Fine.
	2	0	39	53	29,90		WSW	1	Fine.
25	7	0	29	51	30,12		WSW	1	Fine.
	2	0	33	52	30,16		WSW	1	Fine.
26	7	0	33	50	30,26		E	1	Fine.
	2	0	39	52	30,28		ESE	1	Fine.
27	7	0	32	50	30,28		ESE	1	Fine.
	2	0	36	51	30,28		ESE	1	Fine.
28	7	0	30	48	30,28		ESE	1	Fine.
	2	0	36	52	30,30		E	1	Fine.
29	7	0	31	49	30,45		E	1	Foggy.
	2	0	36	51	30,44		E	1	Fine.
30	7	0	29	48	30 38		E	1	Foggy.
	2	0	36	51	30,33		ESE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	34	48	30,17		SE	1	Fine.
	2	0	42	51	30,12		SE	1	Cloudy.
2	8	0	39	50	29,77		SE	1	Cloudy.
	2	0	43	52	29,64		SSE	1	Rainy.
3	8	0	39	50	29,57	0,025	S by W	1	Fine.
	2	0	46	52	29,60		S by W	1	Fine.
4	8	0	39	52	29,57		SW	1	Fine.
	2	0	48	54	29,61		SW	1	Cloudy.
5	8	0	48	53	29,39	0,845	SSW	1	Rainy.
	2	0	48	56	29,40		SW	1	Fair.
6	8	0	38	53	29,56	0,300	NNW	1	Cloudy.
	2	0	46	55	29,67		NNW	1	Cloudy.
7	8	0	37	53	29,88		NNW	1	Fine.
	2	0	47	55	29,94		E	1	Cloudy.
8	8	0	45	54	29,94	0,130	SSE	1	Cloudy.
	2	0	45	56	29,94		S	1	Cloudy.
9	8	0	53	55	29,71	0,440	SSW	2	Cloudy.
	2	0	53	56	29,75		SW	2	Fine.
10	8	0	48	55	29,87		SSW	1	Fine.
	2	0	50	56	29,87		SSW	1	Fine.
11	8	0	50	56	29,77		SSW	1	Cloudy.
	2	0	52	58	29,73		S by W	1	Fine.
12	8	0	48	56	29,47		S	1	Cloudy.
	2	0	53	58	29,53		S	1	Cloudy.
13	8	0	44	57	29,70		WSW	1	Cloudy.
	2	0	52	58	29,75		W	1	Fine.
14	8	0	48	57	29,45	0,195	S	2	Rainy.
	2	0	51	57	29,37		S	1	Fine.
15	8	0	47	58	29,38	0,040	W	1	Foggy.
	2	0	48	59	29,39		SW	1	Cloudy.
16	8	0	46	58	29,45	0,345	W	1	Cloudy.
	2	0	48	59	29,56		W	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Wind.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	42	57	29,53	0,038	E	1	Cloudy.
	2	0	45	57	29,43		E	1	Cloudy.
18	8	0	47	58	29,33	0,073	S	1	Rainy.
	2	0	50	57	29,30		S	1	Cloudy.
19	8	0	46	57	29,47	0,060	S	1	Cloudy.
	2	0	45	57	29,46		S	1	Cloudy.
20	8	0	45	55	29,38	0,100	SSW	1	Foggy.
	2	0	47	58	29,38		WSW	1	Foggy.
21	8	0	38	55	29,76		NE	1	Cloudy.
	2	0	40	55	29,78		NE	1	Cloudy.
22	8	0	35	54	29,70		ENE	1	Cloudy.
	2	0	38	55	29,65		E	1	Cloudy.
23	8	0	31	52	29,55		NE	1	Fine.
	2	0	35	54	29,49		NE	1	Fine.
24	8	0	30	50	29,35		ENE	1	Snow.
	2	0	32	53	29,30		NE	2	Snow.
25	8	0	32	50	29,21		N	2	Snow.
	2	0	33	51	29,33		N	2	Snow.
26	8	0	31	49	29,56		W	1	Cloudy.
	2	0	35	51	29,64		WSW	1	Cloudy.
27	8	0	35	49	29,96		NW	1	Cloudy.
	2	0	37	52	30,13		NNW	1	Fine.
28	8	0	32	48	30,40		N	1	Cloudy.
	2	0	36	52	30,40		N	1	Cloudy.
29	8	0	34	49	30,24		ESE	1	Fine.
	2	0	37	51	30,15		E	1	Cloudy.
30	8	0	37	48	29,91	0,409	E	1	Foggy.
	2	0	37	48	29,90		E	1	Foggy.
31	8	0	37	46	30,04		ESE	1	Foggy.
	2	0	39	50	30,06		ESE	1	Foggy.

1787	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January	51	27	39	54	45	49½	30,64	29,52	30,08	0,360
February	53	31	42	56	50	53	30,36	28,67	29,51	1,041
March	56	34	45	58	51½	54¾	30,51	29,03	29,77	1,283
April	58	37	47½	58	53	55½	30,48	29,07	29,77	0,923
May	71	39	55	64	53	58½	30,31	29,33	29,82	1,137
June	77	46	61½	67	59	63	30,29	29,55	29,92	0,662
July	78	51	64½	71	63	67	30,38	29,37	29,87	3,246
Auguſt	83½	48	65¾	73	62	67½	30,40	29,25	29,82	1,171
September	68	46	57	64	61	62½	30,45	29,11	29,78	0,990
October	63	39	51	62	54	58	30,15	29,21	29,68	1,818
November	56	29	42½	61	48	54½	30,45	29,12	29,78	1,340
December	53	30	41½	59	46	52½	30,40	29,21	29,80	3,000
Whole year			51			58			29,80	16,971

EXPLANATION OF THE INSTRUMENTS.

The inſtruments with which the foregoing obſervations were made are the ſame that were uſed in former obſervations of this kind, a full account of which was given by HENRY CAVENDISH, Eſq. F. R. S. in the LXVIth Volume of the Philoſophical Tranſactions; but as they have been moved from the ſituations they had at that time, it may not be amiſs to mention how they are placed now, in order the better to ſhew what degree of accuracy may be expected from them. There being no one ſituation for a thermometer out of doors ſo good as could be wiſhed, it became neceſſary to make uſe of two thermometers; each is placed out of a three-pair-of-ſtairs window, one facing E.N.E. and the other W.S.W. and they ſtand about two or three inches from the wall, that they may be the more expoſed to the air, and the leſs affected by the heat and cold of the houſe. As the ſun ſhines on the eaſtern part of the building in the morning, the thermometer to the weſtward is made uſe of for the morning obſervation during that ſeaſon of the year when the ſun riſes high enough to affect the other; for all other obſervations, that to the eaſtward is employed. Neither the building oppoſite, nor that on the ſouth ſide of the thermometer to the eaſt, are elevated above it in an angle of more than 13°; but the oppoſite building is not more than twenty-five feet diſtant. The thermometer to the weſtward will not be affected by any other building than one to the northward, which is elevated above it in an angle of 20°, and which is only twenty feet diſtant.

The thermometer within doors is placed cloſe to the barometer, the heights of which it is intended chiefly to correct; the windows of the room in which they are kept look to the weſtward, and in winter the room has conſtantly a fire in it.

The veſſel which receives the rain is fixed to a chimney at the top of the houſe, and riſes ſix inches above the chimney; it is not ſcreened from the rain by any building or chimnies, there being none higher than that to which it is fixed.

END OF PART I. OF VOL. LXXVIII.

PHILOSOPHICAL TRANSACTIONS,

OF THE

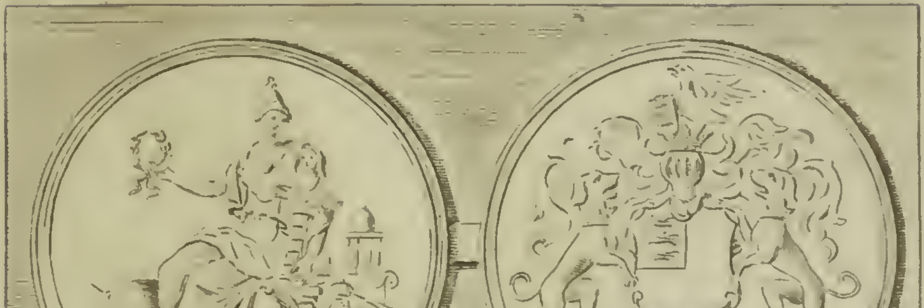
ROYAL SOCIETY

OF

LONDON.

VOL. LXXVIII. For the Year 1788.

PART II.



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PHILOSOPHICAL

TRANSACTIONS.

XIV. *Obſervations on the Natural Hiſtory of the* Cuckoo. *By Mr.* Edward Jenner. *In a Letter to* John Hunter, *Eſq. F. R. S.*

Read March 13, 1788.

TO JOHN HUNTER, ESQ.

DEAR SIR,

HAVING, at your requeſt, employed ſome of my leiſure hours in attending to the natural hiſtory of the Cuckoo, I beg leave to lay before you the reſult of my obſervations, with a hope that they may tend to illuſtrate a ſubject hitherto not ſufficiently inveſtigated; and ſhould what is here offered prove, in your opinion, deſerving the attention of the Royal Society, you will do me the honour of preſenting it to that learned Body.

The firſt appearance of Cuckoos in Glouceſterſhire (the part of England where theſe obſervations were made) is about the 17th of April. The ſong of the male, which is well known, ſoon proclaims its arrival. The ſong of the female (if the peculiar notes of which it is compoſed may be ſo called) is widely different, and has been ſo little attended to, that I believe few are acquainted with it. I know not how to convey to you a proper idea of it by a compariſon with the notes of any other bird; but the cry of the Dab-chick bears the neareſt reſemblance to it.

Unlike the generality of birds, Cuckoos do not pair. When a female appears on the wing, ſhe is often attended by two or three males, who ſeem to be earneſtly contending for her favours. From the time of her appearance, till after the middle of ſummer, the neſts of the birds ſelected to receive her egg are to be found in great abundance; but, like the other migrating birds, ſhe does not begin to lay till ſome weeks after her arrival. I never could procure an egg till after the middle of May, though probably an early-coming Cuckoo may produce one ſooner*.

---

* What is meant by an *early-coming* Cuckoo, I ſhall more fully explain in a Paper I intend to lay before you on the Migration of Birds; but it may be neceſſary to mention here, that migrating birds of the ſame ſpecies arrive and depart in ſucceſſion. Cuckoos, for example, appear in greater numbers on the the ſecond than on the week of their arrival, and they diſappear in the ſame gradual manner.

The Cuckoo makes choice of the neſts of a great variety of ſmall birds. I have known its egg intruſted to the care of the Hedge-ſparrow, the Water-wagtail, the Titlark, the Yellow-hammer, the green Linnet, and the Whinchat. Among theſe it generally ſelects the three former; but ſhews a much greater partiality to the Hedge-ſparrow than to any of the reſt: therefore, for the purpoſe of avoiding confuſion, this bird only, in the following account, will be conſidered as the foſter-parent of the Cuckoo, except in inſtances which are particularly ſpecified.

The Hedge-ſparrow commonly takes up four or five days in laying her eggs. During this time (generally after ſhe has laid one or two) the Cuckoo contrives to depoſit her egg among the reſt, leaving the future care of it entirely to the Hedge-ſparrow. This intruſion often occaſions ſome diſcompoſure; for the old Hedge-ſparrow at intervals, whilſt ſhe is ſitting, not unfrequently throws out ſome of her own eggs, and ſometimes injures them in ſuch a way that they become addle; ſo that it more frequently happens, that only two or three Hedge-ſparrow's eggs are hatched with the Cuckoo's than otherwiſe: but whether this be the caſe or not, ſhe ſits the ſame length of time as if no foreign egg had been introduced, the Cuckoo's egg requiring no longer incubation than her own. However, I have never ſeen an inſtance where the Hedge-ſparrow has either thrown out or injured the egg of the Cuckoo.

When the Hedge-ſparrow has ſat her uſual time, and diſengaged the young Cuckoo and ſome of her own offspring from the ſhell *, her own young ones, and any of her eggs that remain unhatched, are ſoon turned out, the young Cuckoo remaining poſſeſſor of the neſt, and ſole object of her future care.

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* The young Cuckoo is commonly hatched firſt.

care. The young birds are not previouſly killed, nor are the eggs demoliſhed; but all are left to periſh together, either entangled about the buſh which contains the neſt, or lying on the ground under it.

The early fate of the young Hedge-ſparrows is a circumſtance that has been noticed by others, but attributed to wrong cauſes. A variety of conjectures have been formed upon it. Some have ſuppoſed the parent Cuckoo the author of their deſtruction; while others, as erroneouſly, have pronounced them ſmothered by the diſproportionate ſize of their fellow-neſtling. Now the Cuckoo's egg being not much larger than the Hedge-ſparrow's (as I ſhall more fully point out hereafter) it neceſſarily follows, that at firſt there can be no great difference in the ſize of the birds juſt burſt from the ſhell. Of the fallacy of the former aſſertion alſo I was ſome years ago convinced, by having found that many Cuckoo's eggs were hatched in the neſts of other birds after the old Cuckoo had diſappeared; and by ſeeing the ſame fate then attend the neſtling ſparrows as during the appearance of old Cuckoos in this country. But, before I proceed to the facts relating to the death of the young Sparrows, it will be proper to lay before you ſome examples of the incubation of the egg, and the rearing of the young Cuckoo; ſince even the well known fact, that this buſineſs is intruſted to the care of other birds, has been controverted by an Author who has lately written on this ſubject*; and ſince, as it is a fact ſo much out of the ordinary courſe of nature, it may ſtill probably be diſbelieved by others.

---

* The Hon. DAINES BARRINGTON.

EXAMPLE I.

The Titlark is frequently ſelected by the Cuckoo to take charge of its young one; but as it is a bird leſs familiar than many that I have mentioned, its neſt is not ſo often diſcovered. I have, nevertheleſs, had ſeveral Cuckoo's eggs brought to me that were found in Titlark's neſts; and had one opportunity of ſeeing the young Cuckoo in the neſt of this bird: I ſaw the old birds feed it repeatedly, and, to ſatisfy myſelf that they were really Titlarks, ſhot them both, and found them to be ſo.

EXAMPLE II.

A Cuckoo laid her egg in a Water-wagtail's neſt in the thatch of an old cottage. The Wagtail ſat her uſual time, and then hatched all the eggs but one; which, with all the young ones, except the Cuckoo, was turned out of the neſt. The young birds, conſiſting of five, were found upon a rafter that projected from under the thatch, and with them was the egg, not in the leaſt injured. On examining the egg, I found the young Wagtail it contained quite perfect, and juſt in ſuch a ſtate as birds are when ready to be diſengaged from the ſhell. The Cuckoo was reared by the Wagtails till it was nearly capable of flying, when it was killed by an accident.

EXAMPLE III.

A Hedge-ſparrow built her neſt in a hawthorn buſh in a timber-yard: after ſhe had laid two eggs, a Cuckoo dropped in a third.

a third. The Sparrow continued laying, as if nothing had happened, till ſhe had laid five, her uſual number, and then ſat.

June 20, 1786. On inſpecting the neſt I found, that the bird had hatched this morning, and that every thing but the young Cuckoo was thrown out. Under the neſt I found one of the young Hedge-ſparrows dead, and one egg by the ſide of the neſt entangled with the coarſe woody materials that formed its outſide covering. On examining the egg, I found one end of the ſhell a little cracked, and could ſee that the Sparrow it contained was yet alive. It was then reſtored to the neſt, but in a few minutes was thrown out. The egg being again ſuſpended by the outſide of the neſt, was ſaved a ſecond time from breaking. To ſee what would happen if the Cuckoo was removed, I took out the Cuckoo, and placed the egg containing the Hedge-ſparrow in the neſt in its ſtead. The old birds, during this time, flew about the ſpot, ſhewing ſigns of great anxiety; but when I withdrew, they quickly came to the neſt again. On looking into it in a quarter of an hour afterwards, I found the young one completely hatched, warm and lively. The Hedge-ſparrows were ſuffered to remain undiſturbed with their new charge for three hours (during which time they paid every attention to it) when the Cuckoo was again put into the neſt. The old Sparrows had been ſo much diſturbed by theſe intruſions, that for ſome time they ſhewed an unwillingneſs to come to it: however, at length they came, and on examining the neſt again in a few minutes, I found the young Sparrow was tumbled out. It was a ſecond time reſtored, but again experienced the ſame fate.

From theſe experiments, and ſuppoſing, from the feeble appearance of the young Cuckoo juſt diſengaged from the ſhell, that it was utterly incapable of diſplacing either the egg or the young Sparrows, I was induced to believe, that the old Sparrows were the only agents in this ſeeming unnatural buſineſs; but I afterwards clearly perceived the cauſe of this ſtrange phænomenon, by diſcovering the young Cuckoo in the act of diſplacing its fellow-neſtlings, as the following relation will fully evince.

June 18, 1787, I examined the neſt of a Hedge-ſparrow, which then contained a Cuckoo's and three Hedge-ſparrow's eggs. On inſpecting it the day following, I found the bird had hatched, but that the neſt now contained only a young Cuckoo and one young Hedge-ſparrow. The neſt was placed ſo near the extremity of a hedge, that I could diſtinctly ſee what was going forward in it; and, to my aſtoniſhment, ſaw the young Cuckoo, though ſo newly hatched, in the act of turning out the young Hedge-ſparrow.

The mode of accompliſhing this was very curious. The little animal, with the aſſiſtance of its rump and wings, contrived to get the bird upon its back, and making a lodgement for the burden by elevating its elbows, clambered backward with it up the ſide of the neſt till it reached the top, where reſting for a moment, it threw off its load with a jerk, and quite diſengaged it from the neſt. It remained in this ſituation a ſhort time, feeling about with the extremities of its wings, as if to be convinced whether the buſineſs was properly executed, and then dropped into the neſt again. With theſe (the extremities of its wings) I have often ſeen it examine, as it were, an egg and neſtling before it began its operations; and the nice ſenſibility which theſe parts appeared to poſſeſs ſeemed ſufficiently to compenſate the want of ſight, which as yet it was deſtitute of. I afterwards put in an egg, and this, by a ſimilar proceſs, was conveyed to the edge of the neſt, and thrown out. Theſe experiments I have ſince repeated ſeveral times in different neſts, and have always found the young Cuckoo diſpoſed to act in the ſame manner. In climbing up the neſt, it ſometimes drops its burden, and thus is foiled in its endeavours; but, after a little reſpite, the work is reſumed, and goes on almoſt inceſſantly till it is effected. It is wonderful to ſee the extraordinary exertions of the young Cuckoo, when it is two or three days old, if a bird be put into the neſt with it that is too weighty for it to lift out. In this ſtate it ſeems ever reſtleſs and uneaſy. But this diſpoſition for turning out its companions begins to decline from the time it is two or three till it is about twelve days old, when, as far as I have hitherto ſeen, it ceaſes. Indeed, the diſpoſition for throwing out the egg appears to ceaſe a few days ſooner; for I have frequently ſeen the young Cuckoo, after it had been hatched nine or ten days, remove a neſtling that had been placed in the neſt with it, when it ſuffered an egg, put there at the ſame time, to remain unmoleſted. The ſingularity of its ſhape is well adapted to theſe purpoſes; for, different from other newly-hatched birds, its back from the *ſcapulæ* downwards is very broad, with a conſiderable depreſſion in the middle. This depreſſion ſeems formed by nature for the deſign of giving a more ſecure lodgement to the egg of the Hedge-ſparrow, or its young one, when the young Cuckoo is employed in removing either of them from the neſt. When it is about twelve days old, this cavity is quite filled up, and then the back aſſumes the ſhape of neſtling birds in general.

Having found that the old Hedge-ſparrow commonly throws out ſome of her own eggs after her neſt has received the Cuckoo's, and not knowing how ſhe might treat her young ones if the young Cuckoo was deprived of the power of diſpoſſeſſing them of the neſt, I made the following experiment.

July 9. A young Cuckoo, that had been hatched by a Hedge-ſparrow about four hours, was confined in the neſt in ſuch a manner that it could not poſſibly turn out the young Hedge-ſparrows which were hatched at the ſame time, though it was almoſt inceſſantly making attempts to effect it. The conſequence was, the old birds fed the whole alike, and appeared in every reſpect to pay the ſame attention to their own young as to the young Cuckoo, until the 13th, when the neſt was unfortunately plundered.

The ſmallneſs of the Cuckoo's egg in proportion to the ſize of the bird is a circumſtance that hitherto, I believe, has eſcaped the notice of the ornithologiſt. So great is the diſproportion, that it is in general ſmaller than that of the Houſe-ſparrow; whereas the difference in the ſize of the birds is nearly as five to one. I have uſed the term *in general*, becauſe eggs produced at different times by the ſame bird vary very much in ſize. I have found a Cuckoo's egg ſo light that it weighed only forty-three grains, and one ſo heavy that it weighed fifty-five grains. The colour of the Cuckoo's eggs is extremely variable. Some, both in ground and penciling, very much reſemble the Houſe-ſparrow's; ſome are indiſtinctly covered with bran-coloured ſpots; and others are marked with lines of black, reſembling, in ſome meaſure, the eggs of the Yellow-hammer.

The circumſtance of the young Cuckoo's being deſtined by nature to throw out the young Hedge-ſparrows, ſeems to account for the parent-cuckoo's dropping her egg in the neſts of birds ſo ſmall as thoſe I have particulariſed. If ſhe were to do this in the neſt of a bird which produced a large egg, and conſequently a large neſtling, the young Cuckoo would probably find an inſurmountable difficulty in ſolely poſſeſſing the neſt, as its exertions would be unequal to the labour of turning out the young birds*. Beſides, though many of the larger birds might have fed the neſtling Cuckoo very properly, had it been committed to their charge, yet they could not have ſuffered their own young to have been ſacrificed, for the accommodation of the Cuckoo, in ſuch great number as the ſmaller ones, which are ſo much more abundant; for though it would be a vain attempt to calculate the numbers of neſtlings deſtroyed by means of the Cuckoo, yet the ſlighteſt obſervation would be ſufficient to convince us that they muſt be very large.

Here it may be remarked, that though nature permits the young Cuckoo to make this great waſte, yet the animals thus deſtroyed are not thrown away or rendered uſeleſs. At the ſeaſon when this happens, great numbers of tender quadrupeds and reptiles are ſeeking proviſion; and if they find the callow neſtlings which have fallen victims to the young Cuckoo, they are furniſhed with food well adapted to their peculiar ſtate.

It appears a little extraordinary, that two Cuckoo's eggs ſhould ever be depoſited in the ſame neſt, as the young one produced from one of them muſt inevitably periſh; yet I have known two inſtances of this kind, one of which I ſhall relate.

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* I have known an inſtance in which a Hedge-ſparrow ſat upon a Cuckoo's egg and one of her own. Her own egg was hatched five days before the Cuckoo's, when the young Hedge-ſparrow had gained ſuch a ſuperiority in ſize that the young Cuckoo had not powers ſufficient to lift it out of the neſt till it was two days old, by which time it was grown very conſiderably. This egg was probably laid by the Cuckoo ſeveral days after the Hedge-ſparrow had begun to ſit; and even in this caſe it appears, that its preſence had created the diſturbance before alluded to, as all the Hedge-ſparrow's eggs were gone except one.

June 27, 1787. Two Cuckoos and a Hedge-ſparrow were hatched in the ſame neſt this morning; one Hedge-ſparrow's egg remained unhatched. In a few hours after, a conteſt began between the Cuckoos for the poſſeſſion of the neſt, which continued undetermined till the next afternoon; when one of them, which was ſomewhat ſuperior in ſize, turned out the other, together with the young Hedge-ſparrow and the unhatched egg. This conteſt was very remarkable. The combatants alternately appeared to have the advantage, as each carried the other ſeveral times nearly to the top of the neſt, and then ſunk down again, oppreſſed by the weight of its burden; till at length, after various efforts, the ſtrongeſt prevailed, and was afterwards brought up by the Hedge-ſparrows.

I come now, Sir, to conſider the principal matter that has agitated the mind of the naturaliſt reſpecting the Cuckoo: *why, like other birds, it ſhould not build a neſt, incubate its eggs, and rear its own young?*

There is certainly no reaſon to be aſſigned from the formation of this bird why, in common with others, it ſhould not perform all theſe ſeveral offices; for it is in every reſpect perfectly formed for collecting materials and building a neſt. Neither its external ſhape nor internal ſtructure prevent it from incubation; nor is it by any means incapacitated from bringing food to its young. It would be needleſs to enumerate the various opinions of authors on this ſubject from ARISTOTLE to the preſent time. Thoſe of the ancients appear to be either viſionary, or erroneous; and the attempts of the moderns towards its inveſtigation have been confined within very narrow limits; for they have gone but little farther in their reſearches than to examine the conſtitution and ſtructure of the bird, and having found it poſſeſſed of a capacious ſtomach with a thin external covering, concluded that the preſſure upon this part, in a ſitting poſture, prevented incubation. They have not conſidered that many of the birds which incubate have ſtomachs analogous to thoſe of Cuckoos: the ſtomach of the Owl, for example, is proportionably capacious, and is almoſt as thinly covered with external integuments. Nor have they conſidered, that the ſtomachs of neſtlings are always much diſtended with food; and that this very part, during the whole time of their confinement to the neſt, ſupports, in a great degree, the weight of the whole body; whereas, in a ſitting bird, it is not nearly ſo much preſſed upon; for the breaſt in that caſe fills up chiefly the cavity of the neſt, for which purpoſe, from its natural convexity, it is admirably well fitted.

Theſe obſervations, I preſume, may be ſufficient to ſhew that the Cuckoo is not rendered incapable of ſitting through a peculiarity either in the ſituation or formation of the ſtomach; yet, as a proof ſtill more deciſive, I ſhall lay before you the following fact.

In the ſummer of the year 1786, I ſaw, in the neſt of a Hedge-ſparrow, a Cuckoo, which, from its ſize and plumage, appeared to be nearly a fortnight old. On lifting it up in the neſt, I obſerved two Hedge-ſparrow's eggs under it. At firſt I ſuppoſed them part of the number which had been ſat upon by the Hedge-ſparrow with the Cuckoo's egg, and that they had become addle, as birds frequently ſuffer ſuch eggs to remain in their neſts with their young; but on breaking one of them I found it contained a living fœtus; ſo that of courſe theſe eggs muſt have been laid ſeveral days after the Cuckoo was hatched, as the latter now completely filled up the neſt, and was by this peculiar incident performing the part of a ſitting-bird *.

Having under my inſpection, in another Hedge-ſparrow's neſt, a young Cuckoo, about the ſame ſize as the former, I procured two Wagtail's eggs which had been ſat upon a few days, and had them immediately conveyed to the ſpot, and placed under the Cuckoo. On the ninth day after the eggs had been in this ſituation, the perſon appointed to ſuperintend the neſt (as it was ſome diſtance from the place of my reſidence) came to inform me, that the Wagtails were hatched. On going to the place, and examining the neſt, I found nothing in it but the Cuckoo and the ſhells of the Wagtail's eggs. The fact, therefore, of the birds being hatched, I do not give you as coming immediately under my own eye; but the teſtimony of the perſon appointed to watch the neſt was corroborated by that of another witneſs.

To what cauſe then may we attribute the ſingularities of the Cuckoo? May they not be owing to the following circumſtances? *The ſhort reſidence this bird is allowed to make in the country where it is deſtined to propagate its ſpecies, and the call that nature has upon it, during that ſhort reſidence, to produce a numerous progeny.* The Cuckoo's firſt appearance here is about the middle of April, commonly on the 17th. Its egg is not ready for incubation till ſome weeks after its arrival, ſeldom before the middle of May. A fortnight is taken up by the ſitting bird in hatching the egg. The young bird generally continues three weeks in the neſt before it flies, and the foſter-parents feed it more than five weeks after this period; ſo that, if a Cuckoo ſhould be ready with an egg much ſooner than the time pointed out, not a ſingle neſtling, even one of the earlieſt, would be fit to provide for itſelf before its parent would be inſtinctively directed to ſeek a new reſidence, and be thus compelled to abandon its young one; for old Cuckoos take their final leave of this country the firſt week in July.

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* At this time I was unacquainted with the fact, that the young Cuckoo turned out the eggs of the Hedge-ſparrow; but it is reaſonable to conclude, that it had loſt the diſpoſition for doing this when theſe eggs were depoſited in the neſt.

Had nature allowed the Cuckoo to have ſtaid here as long as ſome other migrating birds, which produce a ſingle ſet of young ones (as the Swift or Nightingale, for example), and had allowed her to have reared as large a number as any bird is capable of bringing up at one time, theſe might not have been ſufficient to have anſwered her purpoſe; but by ſending the Cuckoo from one neſt to another, ſhe is reduced to the ſame ſtate as the bird whoſe neſt we daily rob of an egg, in which caſe the ſtimulus for incubation is ſuſpended. Of this we have a familiar example in the common domeſtic fowl. That the Cuckoo actually lays a great number of eggs, diſſection ſeems to prove very deciſively. Upon a compariſon I had an opportunity of making between the ovarium, or racemus vitellorum, of a female Cuckoo, killed juſt as ſhe had begun to lay, and of a pullet killed in the ſame ſtate, no eſſential difference appeared. The uterus of each contained an egg perfectly formed, and ready for excluſion; and the ovarium exhibited a large cluſter of eggs gradually advanced from a very diminutive ſize, to the greateſt the yolk acquires before it is received into the oviduct. The appearance of one killed on the third of July was very different. In this I could diſtinctly trace a great number of the membranes which had diſcharged yolks into the oviduct; and one of them appeared as if it had parted with a yolk the preceding day. The ovarium ſtill exhibited a cluſter of enlarged eggs; but the moſt forward of them was ſcarcely larger than a muſtard-ſeed.

I would not be underſtood, Sir, to advance that every egg which ſwells in the ovarium at the approach or commencement of the propagating ſeaſon is brought to perfection; but it appears clearly, that a bird, in obedience to the dictates of her own will, or to ſome hidden cauſe in the animal œconomy, can either retard or bring forward her eggs. Beſides the example of the common fowl above alluded to, many others occur. If you deſtroy the neſt of a Blackbird, a Robin, or almoſt any ſmall bird, in the ſpring, when ſhe has laid her uſual number of eggs, it is well known to every one, who has paid any attention to enquiries of this kind, in how ſhort a ſpace of time ſhe will produce a freſh ſet. Now, had the bird been ſuffered to have proceeded without interruption in her natural courſe, the eggs would have been hatched, and the young ones brought to a ſtate capable of providing for themſelves, before ſhe would have been induced to make another neſt, and excited to produce another ſet of eggs from the ovarium. If the bird had been deſtroyed at the time ſhe was ſitting on her firſt laying of eggs, diſſection would have ſhewn the ovarium containing a great number in an enlarged ſtate, and advancing in the uſual progreſſive order. Hence it plainly appears, that birds can keep back or bring forward (under certain limitations) their eggs at any time during the ſeaſon appointed for them to lay; but the Cuckoo, not being ſubject to the common interruptions, goes on laying from the time ſhe begins, till the eve of her departure from this country: for although old Cuckoos in general take their leave the firſt week in July (and I never could ſee one after the 5th day of that month [*]), yet I have known an inſtance of an egg's being hatched in the neſt of a Hedge-ſparrow ſo late as the 15th. And a farther proof of their continuing to lay till the time of their leaving us may, I think, be fairly deduced from the appearances on diſſection of the female Cuckoo above-mentioned, killed on the 3d of July.

Among the many peculiarities of the young Cuckoo, there is one that ſhews itſelf very early. Long before it leaves the neſt, it frequently, when irritated, aſſumes the manner of a bird of prey, looks ferocious, throws itſelf back, and pecks at any thing preſented to it with great vehemence, often at the ſame time making a chuckling noiſe like a young hawk. Sometimes, when diſturbed in a ſmaller degree, it makes a kind of hiſſing noiſe, accompanied with a heaving motion of the whole body [†]. The growth of the young Cuckoo is uncommonly rapid.

The chirp is plaintive, like that of the Hedge-ſparrow; but the ſound is not acquired from the foſter-parent, as it is the ſame whether it be reared by the Hedge-ſparrow, or any other bird.

It never acquires the adult note during its ſtay in this country.

The ſtomachs of young Cuckoos contain a great variety of food. On diſſecting one that was brought up by Wagtails, and fed by them at the time it was ſhot (though it was nearly of the ſize and fulneſs of plumage of the parent-bird) I found in its ſtomach the following ſubſtances.

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[*] Though I am unacquainted with an inſtance, yet I conceive it poſſible, that here and there a ſtraggling Cuckoo may be ſeen after this time.

[†] Young animals, being deprived of other modes of defence, are probably endowed with the powers of exciting fear in their common enemies. If you but ſlightly touch the young Hedge-hog, for inſtance, before it becomes fully armed with its prickly coat, the little animal jumps up with a ſudden ſpring, and imitates very cloſely the ſound of the word *huſh!* as we pronounce it in a loud whiſper. This diſpoſition is apparent in many other animals.

Flies and Beetles of various kinds.

Small Snails, with their ſhells unbroken.

Graſhoppers.

Caterpillars.

Part of a Horſe-bean.

A vegetable ſubſtance reſembling bits of tough graſs, rolled into a ball.

The ſeeds of a vegetable that reſembled thoſe of the gooſe-graſs.

In the ſtomach of one fed by Hedge-ſparrows, the contents were almoſt entirely vegetable; ſuch as wheat, ſmall vetches, &c. But this was the only inſtance of the kind I had ever ſeen, as theſe birds, in general, feed the young Cuckoo with ſcarcely any thing but animal food. However, it ſerved to clear up a point which before had ſomewhat puzzled me; for having found the Cuckoo's egg in the neſt of a green linnet, which begins very early to feed its young with vegetable food, I was apprehenſive, till I ſaw this fact, that this bird would have been an unfit foſter-parent for the young Cuckoo.

The Titlark, I obſerve, feeds it principally with Graſhoppers.

But the moſt ſingular ſubſtance, ſo often met with in the ſtomachs of young Cuckoos, is a ball of hair curiouſly wound up. I have found it of various ſizes, from that of a pea to that of a ſmall nutmeg. It ſeems to be compoſed chiefly of Horſe-hairs, and from the reſemblance it bears to the inſide covering of the neſt, I conceive the bird ſwallows it while a neſtling. In the ſtomachs of old Cuckoos I have often ſeen

maſſes

maſſes of hair; but theſe had evidently once formed a part of the hairy Caterpillar, which the Cuckoo often takes for its food.

There ſeems to be no preciſe time fixed for the departure of young Cuckoos. I believe they go off in ſucceſſion, probably as ſoon as they are capable of taking care of themſelves; for although they ſtay here till they become nearly equal in ſize and growth of plumage to the old Cuckoo, yet in this very ſtate the foſtering care of the Hedge-ſparrow is not withdrawn from them. I have frequently ſeen the young Cuckoo of ſuch a ſize that the Hedge-ſparrow has perched on its back, or half-expanded wing, in order to gain ſufficient elevation to put the food into its mouth. At this advanced ſtage, I believe that young Cuckoos procure ſome food for themſelves; like the young rook, for inſtance, which in part feeds itſelf, and is partly fed by the old ones till the approach of the pairing ſeaſon. If they did not go off in ſucceſſion, it is probable we ſhould ſee them in large numbers by the middle of Auguſt; for as they are to be found in great plenty *, when in a neſtling ſtate, they muſt now appear very numerous, ſince all of them muſt have quitted the neſt before this time. But this is not the caſe; for they are not more numerous at any ſeaſon than the parent birds are in the months of May and June.

The ſame inſtinctive impulſe which directs the Cuckoo to depoſit her eggs in the neſts of other birds, directs her young one to throw out the eggs and young of the owner of the neſt. The ſcheme of nature would be incomplete without it; for it would be extremely difficult, if not impoſſible, for the little birds, deſtined to find ſuccour for the Cuckoo, to find it alſo

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* I have known four young Cuckoos in the neſts of Hedge-ſparrows in a ſmall paddock at the ſame time.

for their own young ones, after a certain period; nor would there be room for the whole to inhabit the neſt.

Thus, Sir, I have, with much pleaſure, complied with your requeſt; and here lay before you ſuch obſervations as I have hitherto been capable of making on the natural hiſtory of the Cuckoo; and ſhould they throw ſome light on a ſubject that has long lain hid in obſcurity, I ſhall not think my time has been ill employed.

With a grateful ſenſe of the many obligations I owe to the friendſhip with which you have ſo long honoured me,

I remain, &c.

EDW. JENNER.

Berkeley,
December 27, 1787.

XV. *Of the Temperament of thoſe muſical Inſtruments, in which the Tones, Keys, or Frets, are fixed, as in the Harpſichord, Organ, Guitar,* &c. *By Mr.* Tiberius Cavallo, *F. R. S.*

Read April 3, 1788.

THE ſcale of muſic, which is uſed at preſent, conſiſts of ſeven principal notes or ſounds, which muſicians denote by the letters of the alphabet A, B, C, D, E, F, and G; which, together with ſome intermediate ones, commonly called flats and ſharps, and the octave of the firſt, make 13 ſounds.

When thoſe ſounds are conſidered with reſpect to the firſt, they are called by the following names, *viz.* the prime or key-note, the ſecond minor, ſecond, third minor, third major, fourth, fourth major, fifth, ſixth minor, ſixth major, ſeventh minor, ſeventh major, and octave.

Muſical ſounds are produced by the vibrations of the ſonorous bodies, and they are acuter or graver as the vibrations performed in a given time are more or leſs in number; ſo that if a ſtring vibrating 100 times in a ſecond produces a certain ſound, and another ſtring vibrating 120 times in a ſecond produces another ſound, the latter is ſaid to be acuter, higher, or ſharper than the former.

The number of vibrations performed in a certain time principally depends on the thickneſs, length, and elaſticity of the ſonorous bodies; but as the ſimpleſt ſonorous bodies, and the fitteſt for examination, are thoſe ſtrings which are equal in every

other

other reſpect, excepting in their lengths, becauſe the number of vibrations, which they perform in a given time, is ſimply in the proportion of their lengths, we ſhall conſider only thoſe in the preſent inveſtigation, the number of vibrations performed by other ſorts of ſonorous bodies being eaſily deduced from them.

As the above-mentioned 13 ſounds are all different from each other, the ſtrings which produce them differ in length, and of courſe in the number of the vibrations, which, when ſtruck, they perform in a certain time. Here follow the proportions which the numbers of vibrations performed in a given time, or the length of the ſtrings which expreſs thoſe 13 ſounds, bear to the firſt, prime, or key-note.

Firſt	1	Fourth	$\frac{3}{4}$	Seventh minor	$\frac{5}{9}$
Second minor	$\frac{15}{16}$	Fourth major	$\frac{32}{45}$	Seventh major	$\frac{8}{15}$
Second	$\frac{8}{9}$	Fifth	$\frac{2}{3}$	Octave	$\frac{1}{2}$
Third minor	$\frac{5}{6}$	Sixth minor	$\frac{5}{8}$		
Third major	$\frac{4}{5}$	Sixth major	$\frac{3}{5}$		

If, inſtead of many ſtrings having thoſe lengths in order to expreſs the 13 ſounds, or notes of an octave, one ſtring be divided according to thoſe proportions, and this ſtring be ſtopped conſecutively in the different points or diviſions; on being ſtruck, it will expreſs the correſponding ſounds. Thus, if a ſtring ſtretched between two fixed points, as CZ fig. 1. Tab. III. be ſtruck, it will produce a ſound called the prime, firſt or key-note; if it be ſtopped in the middle, one-half of the ſtring will ſound the octave, its length, compared to that of the whole ſtring, being in the proportion of 2 to 1; if two-thirds of the ſtring be cauſed to vibrate, the ſound produced will be the fifth, its length, compared to that of the whole ſtring, being as 2 to 3, and ſo of the reſt.

The higheſt ſound of the octave is expreſſed by the half of the ſtring; and if this half be divided again in the ſame manner or proportion, a higher octave will be obtained, the higheſt note of which will be expreſſed by a quarter of the original ſtring. This quarter may be divided again into a higher octave, and ſo on; therefore, a ſtring ſo divided may expreſs the ſounds of all the keys of an harpſichord or organ.

In regard to thoſe diviſions it muſt be obſerved, that as the notes of the ſecond octave bear the ſame proportion to the firſt note of that octave as the notes of the firſt octave reſpectively bear to the firſt note of that octave, or to the whole ſtring; and as the length of the ſtring expreſſing the firſt note of the ſecond octave, *viz.* *cZ*, fig. 1. is half the length of CZ, the firſt note of the firſt octave, it follows, that the length of the ſtring of every note in the ſecond octave is half the length of the correſponding note in the firſt octave; thus *g* in the ſecond octave is 120 inches long, and G in the firſt octave is 240 inches long, *viz.* twice 120. Hence, when the diviſions of the firſt octave are aſcertained, in order to find the diviſions of the notes of the ſecond octave, we need only take the half of the lengths expreſſing the notes in the firſt octave. By the very ſame reaſoning it is evident, that to find the diviſions for the third octave, we need only take the halves of the lengths which expreſs the notes of the ſecond octave, or the quarters of thoſe of the firſt octave, and ſo of the reſt.

The ſtring or line CZ, fig. 1. is divided in the above-mentioned manner, and in order to avoid confuſion, the diviſions of the principal notes only of the firſt and ſecond octave are annexed to it. The numbers under the line expreſs the lengths from Z to the diviſions to which they ſtand near. The letters juſt over the lines are the names of the notes or ſounds expreſſed by the correſponding lengths of the ſtring. The fractional numbers expreſs the proportion which each particular diviſion bears to the whole ſtring; and the Roman numbers denote the numerical names of each note with reſpect to its diſtance from the firſt, which is always included. For example, ſuppoſe the whole ſtring to be called C, and to be 360 inches long: then, if this ſtring be ſtopped in G, the part GZ will be 240 inches, *viz.* two-thirds of the whole ſtring CZ; the ſound expreſſed by it when ſtruck is called G, and it is the fifth note from C, which is the firſt or key-note. Again, if this ſtring be ſtopped in A, the part AZ will be equal to 216 inches, *viz.* three-fifths of the whole; the ſound produced by it is called A, and is a ſixth to the key-note C, &c.

It is evident, that if any of thoſe diviſions be conſidered as the firſt or key-note, then the other notes, though they retain their alphabetical names, muſt have their numerical names altered accordingly: for example, if we take D for the key-note, then A will be the fifth of it, whereas A was the ſixth when C was conſidered as the key-note; thus alſo B is the third of G, and the ſeventh of C, and ſo on.

Thus much having been premiſed, we may proceed to ſhew the meaning of what is called *the temperament* in a ſyſtem of muſical ſounds, and likewiſe the neceſſity of it. For this purpoſe it is neceſſary to recollect, firſt, that the ſtring, divided in the above-mentioned manner, exhibits the various notes or ſounds of the keys of an harpſichord, the pipes of an organ, &c. Secondly, that thoſe diviſions remain unalterable, ſo that the harpſichord, when tuned, cannot be altered in the courſe of performing on it; and, thirdly, that when any one of thoſe notes or diviſions is conſidered as the key-note, its ſecond, third, fourth, fifth, &c. muſt bear their reſpective proportions, according to what has been ſaid in the preceding pages.

Now, if amongſt the diviſions of the ſtring CZ, fig. 1. we take D for the firſt or key-note, its length being 320 inches, the length of its fifth muſt be 213⅓ inches, *viz.* two thirds of 320, that being the proportion which the fifth muſt bear to the key-note; but amongſt the diviſions of the ſtring, there is none equal to 213⅓ inches; therefore, there is not a note among them which may ſerve for a fifth to D: however, as the length of AZ, *viz.* 216, is the neareſt to 213⅓, this A muſt be taken for the fifth of D. It is evident, that this is an imperfect fifth of D; but if, in order to render it perfect, we make AZ equal to 213⅓ inches inſtead of 216, then it will be a redundant ſixth to C, when C is conſidered as the key-note; the beſt expedient, therefore, is to divide the imperfection between the two lengths, *viz.* to make AZ neither ſo long as 216, nor ſo ſhort as 213⅓, which will render the diſagreeable ſenſation, ariſing from the improper length, the leaſt poſſible. This alteration of the juſt lengths of ſtrings, neceſſary for adapting them to ſeveral key-notes, is called *the temperament:* and the beſt temperament in a ſet of muſical ſounds is evidently ſuch a partition of the natural imperfections, as will render all the chords equally and the leaſt diſagreeable poſſible.

What has been exemplified in D and A may be ſaid of all the other notes; ſo that if any one of them be a perfect third, fifth, &c. with reſpect to one key-note, it will be found to be imperfect with reſpect to others. Hence it is manifeſt, firſt, that in a ſet of muſical keys, pipes, or frets, a temperament is abſolutely neceſſary; and, ſecondly, that the harpſichord, organ, guitar, or any other inſtrument in which the notes are fixed, ſo as not to be alterable by the performer's hands, muſt be imperfect even when tuned in the beſt manner poſſible; for by the temperament we can divide, but not annihilate, the imperfection.

Other inſtruments, in which the notes are not fixed, as the violin, violoncello, &c. are perfect, becauſe the performer ſtops the ſtrings upon them in different places, even for ſounding the notes of the ſame name. Thus a ſkilful performer, in order to ſound A, will ſtop the ſtring a little farther from the bridge when he plays in the key of C, *viz.* when C is conſidered as the key-note, than when he plays in the key of D.

Moſt people imagine, that the ſcale of muſic is capable of many different temperaments; and, agreeable to this ſuppoſition, the writers on harmonics have propoſed different temperaments; but it will be ſhewn in the ſequel, that the nature of the ſcale admits of only one temperament capable of rendering the imperfection and the harmony equal throughout; and that it is impoſſible to form a different and more advantageous ſcale.

Before we begin with the inveſtigation of this ſubject, it will be neceſſary to explain certain principles, the want of which may poſſibly raiſe ſome doubts in the minds of thoſe perſons who are not much acquainted with the theory of muſical ſounds. In the firſt place it muſt be obſerved, that the proportion of 2 to 3 for the fifth, the proportion of 1 to 2 for the octave, and in ſhort the proportions of all the notes, are not aſſumed at pleaſure; but they have been determined from conſtant experience, *viz.* from the agreeable or diſagreeable effects produced when two different notes are ſounded at the ſame time.

To render this more evident, let two ſtrings equal in every reſpect be ſtruck at the ſame time, and they will expreſs the ſame ſound preciſely, ſo that no ear can perceive any difference between them, and it is almoſt impoſſible to diſtinguiſh whether the ſound ariſes from two ſtrings, or from one only, excepting from the loudneſs. But if one of thoſe ſtrings be ſucceſſively ſtopped in different parts of its length, whilſt the other remains open as before, and if at every time they be both ſtruck together, their combined ſounds will be found to produce different effects, *viz.* ſometimes more or leſs pleaſing, and at other times more or leſs diſagreeable. When the combinations of the two ſounds are agreeable, they are called *concords*; and when diſagreeable, they are called *diſcords.*

Experience evinces, that the beſt concord is when the length of one ſtring is to the length of the other as 1 to 2, every other circumſtance being the ſame in both. This proportion forms the octave. The next beſt concord is the fifth, *viz.* when the lengths of the two ſtrings are as 2 to 3, after which come the proportions of 3 to 4, 4 to 5, 3 to 5, 5 to 6, and 5 to 8, for the other concords. The other proportions beſides theſe are diſagreeable in a greater or leſs degree, unleſs they are greater than the proportion of 1 to 2; but in that caſe it will be found, that the proportions which produce agreeable combinations are the double, quadruple, octuple, &c. of thoſe mentioned above, *viz.* are their octaves, double octaves, &c.: thus the proportion of 1 to 4 produces a very agreeable concord, becauſe 1 to 4 is the double of 1 to 2, *viz.* it expreſſes a double octave.

Secondly, it appears, from the foregoing obſervations, that if we have the length of a ſtring, or the proportion of a note in any part of the ſtring, we may eaſily find its octaves by taking its double, or its half, or the double of the double, &c.: for inſtance, in fig. 1. if *cZ* be given equal to 90 inches, we may find its octave below by taking twice 90, *viz.* 180, or the octave of this octave, which is 360, *viz.* equal to twice 180, or to four times 90; and, on the other ſide, we may find the octave above of the given note by taking its half, which is 45, &c.

It is now neceſſary to ſhew why within the octave there are admitted only thirteen different notes, *viz.* eight principal ones, and five others, called ſharps and flats.

The line XY, fig. 2. repreſents a muſical ſtring, the length of which is ſuppoſed to be divided into 13286025 equal parts *. On one ſide of this line there are the diviſions of ſeven ſucceſſive octaves, *viz.* the half of XZ, a quarter of it, &c.; and on the other ſide are the diviſions of a ſeries of fifths, *viz.* the fifth of the whole ſtring, the fifth of this fifth, and ſo on, which are found by taking two-thirds of the whole ſtring, then two-thirds of thoſe two thirds, and ſo on.

Here we take notice only of the octaves and fifths, becauſe they are the principal and the beſt concords; ſo that a temperament being required, it is neceſſary firſt to take care, that theſe concords be not rendered inſufferable to the ear, the reſt admitting of a greater latitude in the temperament or deviation from the perfect ſtate. Beſides, it will appear in the ſequel, that all the other notes are derived from the ſeries of ſucceſſive fifths.

In whatever key a piece of muſic is performed, its fifth is the moſt predominant of its concords; and as the notes of muſic muſt be ſo ordered as that, for the ſake of modulation, any note may be conſidered as the key-note; therefore having found the fifth of the whole ſtring by taking two-thirds of its length, which gives a note called G, we muſt ſuppoſe, that this G may be conſidered as the key-note, conſequently muſt find its fifth, which gives D, as ſhewn in the figure, and ſo on, until we find one of thoſe ſucceſſive fifths, which coincides with one of the ſucceſſive octaves; for after that, to find more ſucceſſive fifths would be only repeating the ſame thing over again.

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* This number has been choſen, becauſe the line may in that caſe be divided into the neceſſary number of ſucceſſive octaves and fifths, without any common fractions, which renders the operation more eaſy and more perſpicuous; otherwiſe there might have been aſſumed any other number.

Indeed, if we carry the ſucceſſion of octaves and of fifths indefinitely far, we ſhall find, that no one of the fifths ever coincides perfectly with one of the octaves, and therefore the diviſion would have no end. However, as the length of the ſeventh octave comes ſo very near to the twelfth fifth, we muſt be contented with taking this ſeventh octave for the fifth of F, the difference between this note and the perfect fifth of F being about the hundredth part of its length; whereas, if we carry on the ſucceſſion of fifths and of octaves, we ſhall find, that amongſt thirty and more fifths none comes nearer to one of the octaves than the above-mentioned one, as may be ſeen in the following table, which contains a ſeries of ſucceſſive octaves, and another ſeries of ſucceſſive fifths, as in fig. 2.; but in the table the diviſions are carried farther on than in the figure:

Octaves.	Fifths.
13286025	13286025.
6643012,5	8857350.
3321506,25	5904900.
1660753,125	3936600.
830376,5625	2624400.

Octaves

Octaves.	Fifths.
415188,28125	1749600.
207594,140625	1166400.
☞103797,0703125	777600.
51898,5351 +	518400.
25949,2675 +	345600.
12974,6387 +	230400.
6487,3193 +	153600.
3243,654 +	☞102400.
1621,827 +	68266,6 +
810,913 +	45511 +
405,456 +	30340,7 +
202,728 +	20227 +
101,364 +	6742,3 +
50,662 +	4494,8 +
25,331 +	2996,6 +
12,665 +	1997,7 +
	1331,8 +
	887,8 +
	592 +
	394,6 +
	263 +
	175,4 +
	117 +
	78 +
	52 +
	34,6 +
	23 +
	15,3 +

The number of fifths then in this ſeries is twelve, and as, from what has been ſaid above, when the diviſion expreſſing a certain note has been aſſigned in any part of a ſtring, we may eaſily find all its octaves above and below, it follows, that by finding all the octaves of thoſe twelve diviſions we ſhall have twelve diſtinct notes within half the ſtring, *viz.* within the firſt octave of the whole ſtring; to which, if the ſound of the whole ſtring be added, we ſhall have thirteen different ſounds, which ſhews why an octave comprehends neither more nor leſs than thirteen notes.

Without dwelling any longer upon the names or number of thoſe notes, I ſhall immediately proceed to find out the temperament.

It has been ſhewn above, and it is expreſſed in fig. 2. that the length of the ſtring for the laſt fifth is ſhorter than the length of the laſt octave, and alſo that one of them muſt be neceſſarily taken for both purpoſes; but here we muſt conſult nature, examining by the ear which of the two is leaſt diſagreeable. This, however, is ſoon decided; for imperfect octaves are quite inſufferable, whereas a certain degree of imperfection in the fifths is tolerable; therefore we are neceſſitated to leave the octaves perfect, and to let the ſeventh octave ſerve for the fifth of F. In this caſe it is evident, that each of the notes in the ſucceſſion of fifths is a perfect fifth to its preceding note, excepting the laſt, which would be by much too flat, and therefore it is neceſſary to divide the imperfection equally among them all.

For this purpoſe it muſt be conſidered, that as the twelve ſucceſſive fifths, together with the whole ſtring or firſt note, are each two-thirds of its preceding note; they form a geometrical ſeries, the ratio of which is $\frac{2}{3}$, its extremes are 13286025 and 102400, and the number of terms is 13. But becauſe inſtead of 102400, which is the laſt fifth, we muſt take the number 103797,0703125 (*viz.* the length of the laſt octave) for the laſt term of the ſeries; therefore the problem is reduced to the finding out of eleven mean proportionals between the two numbers 13286025 and 103797,0703125.

It is demonſtrated in almoſt every treatiſe on algebra and arithmetic, that in a geometrical progreſſion, as the above-mentioned one, the firſt or ſmalleſt extreme is to the laſt or greateſt extreme as unity is to a power of the ratio, the index of which is equal to the number of terms leſs one. Hence, in our caſe, in which the number of terms, including the two extremes, is 13, we ſhall have $103797,0703125 : 13286025 :: 1 : R^{12}$, from which the ratio is found by dividing the ſecond number by the firſt, and extracting the twelfth root from the quotient, *viz.* $\frac{13286025}{103797,0703125} = 128$; and $\overline{128}|^{\frac{1}{12}} = 1,4983069$, which is the ratio ſought.

The ratio having been aſcertained, the ſucceſſion of tempered fifths is thus eaſily determined; *viz.* divide the length of the whole ſtring by this ratio, and the quotient gives the firſt tempered fifth; divide this fifth by the ſame ratio, and the quotient gives the ſecond tempered fifth; divide this ſecond fifth by the ſame ratio, and ſo on till the laſt fifth, which comes out equal to 103797, 21735 (ſee fig. 3.) which is ſo nearly equal to the length of the ſeventh octave, that the difference is truly inſignificant; but, if greater accuracy were required, we need only extract the proper root of 128 to a greater number of decimals.

Fig. 3. ſhews the diviſions of the ſtring XZ tempered in the above-mentioned manner; *viz.* the ſucceſſive fifths have been aſcertained firſt, and then, by taking their octaves, the whole ſet of diviſions has been completed. By comparing this figure with fig. 2. one may eaſily perceive, how ſmall is

the difference between the perfect fifths of the latter, and the tempered ones of the former.

The diviſions, thus aſcertained, form a ſeries of notes, in which the octaves only are perfect; but all the fifths, all the thirds, and in ſhort all the chords of the ſame denomination, are equally tempered throughout: ſo that whichever of them is taken for the key-note, its fifth, ſixth, &c. will have always the ſame proportion to it, and conſequently will always produce the ſame harmony when ſounded with it.

It is evident, that, beſides this, there can be no other temperament capable of producing equal harmony; for when the extremes of a geometrical ſeries and number of mean proportionals are given, there can be but one ſet of thoſe means: thus, if we are to find two mean proportionals between the numbers 2 and 16, theſe are neceſſarily 4 and 8; nor is it poſſible to aſſign any others.

If, on the other hand, we endeavour to find a better temperament by introducing more than thirteen notes within the limits of an octave, we ſhall find it impracticable, becauſe it has been ſhewn, in the preceding pages, that after the number thirteen, if the ſucceſſion of fifths be carried farther on, they will recede more from a coincidence with any one of the octaves.

This explanation of the nature, origin, and neceſſity of the temperament has been thought neceſſary for the ſake of perſpicuity; but the ſame end may be obtained by the following eaſier method. As the thirteen notes of an octave muſt be arranged ſo, that whichever of them be taken for the firſt or key-note, the ſecond, third, fourth, &c. may bear the ſame conſtant proportion to it; therefore it follows, that they muſt be in a geometrical proportion one of the other, ſo as to form a ſeries of thirteen numbers, the extremes of which are the whole ſtring and its half, *viz.* any number and its half. The ratio of this ſeries is found in the ſame manner as in the other ſeries, *viz.* the greateſt extreme is divided by the leaſt, and the twelfth root of the quotient is the ratio ſought. But the extremes are any aſſumed number and its half: and as the quotient of a number divided by the half of the ſame number is always equal to two; therefore, whatever be the length of the ſtring, the ratio is always $\overline{2}|^{\frac{1}{12}} = 1,0594+$, and if the length of the whole ſtring be divided by this ratio, *viz.* 1,0594+, the quotient will be the length of the ſtring expreſſing the ſecond note, which, divided by the ſame ratio, gives the third note, and ſo on; or elſe, inſtead of dividing the length of the whole ſtring by the ratio, you may multiply the half of it by the ratio, the product of which will give the ſeventh note, which multiplied by the ſame ratio gives the ſixth, and ſo on in a retrograde order, which will give the tempered notes of the octaves as well as the former method. By this means the following diviſions for the notes of an octave have been calculated, the length of the whole ſtring having been ſuppoſed equal to 100000.

I.	100000.
✻ b	94387.
II.	89090.
✻ b	84090.
III.	79370.
IV.	74915.
✻ b	70710.
V.	66743.
✻ b	62997.
VI.	59462.
✻ b	56123.

VII. 52973.
VIII. 50000.

If a monochord be divided in this manner, and a harpſichord tuned by it, this inſtrument will then be tuned ſo, that whichever note be taken for the firſt or key-note, its fifth, ſixth, &c. will produce the ſame effect reſpectively.

Thus far I have endeavoured to explain this ſubject in the moſt familiar manner, avoiding as much as poſſible the mathematical language and ſymbols; having found, by experience, that intricate mathematical diſquiſitions, eſpecially on this ſubject, are underſtood only by a few able mathematicians, but that they are neither comprehended, nor even read, by thoſe who might wiſh to underſtand, or to uſe them. It is now neceſſary to conſider this ſubject with reſpect to the practice.

At preſent, the harpſichords and organs are commonly tuned ſo, that ſome concords are very agreeable to the ear, whilſt others are quite intolerable; or, in other words, when the performer plays in certain keys, the harmony is very pleaſing, in others the harmony is juſt tolerable, and in ſome other keys the harmony is quite diſagreeable.

The beſt keys to be played in are the keys of C, of F, of E flat, of B flat, of G and of D in the major mood, and the keys of C, of D, of A, and of B, in the minor mood. Next to thoſe come the leſs agreeable keys of A, A flat, and E in the major mood; beſides thoſe, the reſt are diſagreeable in a greater or leſs degree, ſo that out of twelve keys*, which, on account of the two moods, *viz.* the major and the minor, become twenty-four, there are hardly fourteen that can be uſed; and for this reaſon moſt of the modern compoſitions in muſic are written in thoſe keys.

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* The octave muſt be excepted, in this reſpect it being the ſame as the firſt note.

So far the common method of tuning anſwers ſome purpoſe; for as long as the performer is to play in certain keys only, it is much better to have them tuned in the moſt advantageous manner, than to let thoſe be tuned in a leſs perfect manner for the ſake of others, which he does not intend to uſe. Hence the great harpſichord players generally have their inſtruments tuned in a peculiar manner, *viz.* ſo as to give the moſt advantageous effect to thoſe concords which they more frequently uſe in their compoſitions. And hence alſo, the harpſichords and organs are always tuned different from each other, unleſs they be tuned by the ſame perſon with equal attention, and without any particular inſtructions.

This practice cannot conveniently be laid aſide, *viz.* when the inſtrument is to be tuned for ſolo playing; and for a certain ſtyle of muſic, it is very proper to tune it ſo as to give the greateſt effect to thoſe combinations of ſounds, which are moſtly uſed in thoſe compoſitions. But the caſe is far different when the inſtrument is to ſerve for accompanying other inſtruments in every ſort of muſic, or the voices of good ſingers; for then the diſagreement becomes very audible; and for this purpoſe the harpſichord or organ ought to be tuned according to the above demonſtrated temperament of equal harmony, which is the only one that can poſſibly take place.

When the compoſitions of old maſters are performed in concert, and with the organ or harpſichord tuned in the common manner, the effect is frequently very diſagreeable. This is particularly the caſe with the ſongs of HANDEL, GALLUPPI, LEO, PERGOLESE, and others, who wrote in a great variety of keys, and very often in thoſe, for which the common way of tuning is not at all calculated.

In order to hear the effect of the above-mentioned temperament of equal harmony, I had a monochord made in a very accurate manner, and upon it I laid down the diviſions for the thirteen notes of an octave properly tempered in the manner explained above. After a great deal of trouble in adjuſting the moveable fret, correcting the diviſions, &c. I at laſt ſucceeded ſo well as to render the diviſions exact within at leaſt the 300th part of an inch, and every part of the inſtrument was rendered ſufficiently ſteady and unalterable.

This being done, I had a large harpſichord, with a ſingle uniſon (in order to judge the better of the effect), tuned very accurately by the help of the monochord. With this inſtrument, in whatever key the performer played, the harmony was perfectly equal throughout, and the effect was the ſame as if one played in the key of E natural on a harpſichord tuned in the uſual manner.

I ſhall, therefore, conclude with ſaying, that when the harpſichord, organ, &c. is to ſerve for ſolo playing, and for a particular ſort of muſic, it is proper to tune in the uſual manner, *viz.* ſo as to give the greateſt effect to thoſe concords which occur more frequently in that ſort of muſic; but that when the inſtrument is to ſerve for accompanying other inſtruments or human voices, and eſpecially when modulations and tranſpoſitions are to be practiſed, then it muſt be tuned according to the temperament of equal harmony, which has been explained in the preceding pages.

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XVI. *Deſcription of a new Electrical Inſtrument capable of collecting together a diffuſed or little condenſed Quantity of Electricity. By Mr.* Tiberius Cavallo, *F. R. S.*

Read April 10, 1788.

ONE of the principal deſiderata in practical electricity has been a method of aſcertaining the preſence and quality of ſuch diffuſed or weak electricity as could not immediately affect an electrometer; of this nature is the electricity produced by effervefcences and other proceſſes, the electricity of the atmoſphere in ſerene and warm weather, &c.

M. Volta's condenſer, which is deſcribed in Volume LXXII. of the Philoſophical Tranſactions, was the firſt attempt of the kind, and, indeed, when this inſtrument is in good order, it anſwers exceedingly well; but the difficulty of conſtructing and of preſerving it, added to the frequent uncertainty of the reſult (it being difficult to ſay, whether in certain caſes the electricity obtained comes from the inſtrument itſelf, or from the ſubſtance in queſtion) have occaſioned its being little, if at all, uſed by thoſe who ſtudy the ſubject of electricity.

Mr. Bennet's doubler, which is deſcribed in Volume LXXVII. of the Philoſophical Tranſactions, was alſo intended to manifeſt ſmall, and otherwiſe unperceivable, quantities of electricity; but from the experiments and obſervations, which I had the honour of laying before the Royal Society in November laſt, it ſeems to be clearly ſhewn, that this doubler cannot be of any uſe, on account of its being naturally always electrified.

In the ſame Paper of laſt November, I likewiſe ſhortly mentioned a method which I had uſed for collecting diffuſed quantities of electricity. Since that time, I have improved the method; and, after ſeveral alterations, have conſtructed an inſtrument for the purpoſe, which, to all my friends who are converſant in electricity, as well as to myſelf, ſeems to be free from all thoſe faults which render M. VOLTA's and Mr. BENNET's inſtruments of little, if at all of any uſe. It ſeems, therefore, that the following deſcription of this new inſtrument may be of uſe to thoſe who are purſuing electrical experiments and inveſtigations.

The properties of this machine, which from its office may be called *a collector of electricity*, are, firſt, that when connected with the atmoſphere, the rain, or in ſhort with any body which produces electricity ſlowly, or which contains that power in a very rarefied manner, it collects the electricity, and afterwards renders both the preſence and quality of it manifeſt, by communicating it to an electrometer. Secondly, This collecting power, by increaſing the ſize of the inſtrument, and eſpecially by uſing a ſecond or ſmaller inſtrument of the like ſort to collect the electricity from the former, may be augmented to any degree. Thirdly, It is conſtructed, managed, and preſerved with eaſe and certainty; and it never gives, nor can it give, an equivocal reſult, as I have proved experimentally, and as will appear by conſidering its conſtruction.

The annexed drawing exhibits two perſpective views of this collector. Fig. 1. (Tab. IV.) ſhews the inſtrument in the ſtate of collecting the electricity; and fig. 2. ſhews it in the ſtate in which the collected electricity is to be rendered manifeſt. An electrometer is annexed to each. The letters of reference indicate the ſame parts in both figures.

ABCD is a flat tin plate, thirteen inches long and eight inches broad; to the two ſhorter ſides of which are ſoldered two tin tubes AD and BC, which are open at both ends. DE and CF are two glaſs ſticks covered with ſealing wax by means of heat, and not by diſſolving the ſealing wax in ſpirits. They are cemented into the lower apertures of the tin tubes, and alſo in the wooden bottom of the frame or machine at E and F, ſo that the tin plate ABCD is ſupported by thoſe glaſs ſticks in a vertical poſition, and is exceedingly well inſulated. GHILKM and NOPV are two frames of wood, which being faſtened to the bottom boards, by means of braſs hinges, may be placed ſo as to ſtand in an upright poſition and parallel to the tin plate, as ſhewn in fig. 1. or they may be opened, and laid upon the table which ſupports the inſtrument, as ſhewn in fig. 2. The inward ſurfaces of thoſe frames from their middle upwards is covered with gilt paper XY; but it would be better to cover them with tin plates, hammered very flat. When the lateral frames ſtand ſtraight up, they do not touch the tin plate; but they ſtand at about one-fifth part of an inch aſunder. They are alſo a little ſhorter than the tin plate, in order that they might not touch the tin tubes AD, BC. In the middle of the upper part of each lateral frame is a ſmall flat piece of wood S and T, with a braſs hook; the uſe of which is to hold up the frames without the danger of their falling down when not required, and at the ſame time it prevents their coming nearer to the tin plate than the proper limit. It is evident, that when the inſtrument ſtands as ſhewn in fig. 1. the gilt ſurface of the paper XY, which covers the inſide of the lateral frames, ſtands contiguous and parallel to the tin plate.

When the inſtrument is to be uſed, it muſt be placed upon a table, a window, or other convenient ſupport, a bottle electrometer is placed near it, and is connected, by means of a wire, with one of the tin tubes AD, BC; and by another conducting communication the tin plate muſt be connected with the electrified ſubſtance, the electricity of which is required to be collected on the plate ABCD: thus, for inſtance, if it be required to collect the electricity of the rain, or of the air, the inſtrument being placed near a window, a long wire muſt be put with one extremity into the aperture A or B of one of the tin tubes, and with the other extremity projecting out of the window. If it be required to collect the electricity produced by evaporation, a ſmall tin pan, having a wire or foot of about ſix inches in length, muſt be put upon one of the tin tubes, ſo that the wire going into the tube the pan may ſtand about two or three inches above the inſtrument. A lighted coal is then put into the pan, and a few drops of water poured upon it will produce the deſired effect. Thus far may ſuffice with reſpect to the mechanical deſcription of the inſtrument: the power and uſe of it will be made apparent by the following experiments.

EXP. I. Communicate to the tin plate ABCD a quantity of electricity, for inſtance, as much as would very ſenſibly affect a common cork-ball electrometer; then, if the lateral frames GHM, NOP, ſtand upright, as in fig. 1. the electrometer W will ſhew no divergency; but if the frames are opened and let down, as in fig. 2. the balls of the electrometer W will immediately repel each other, and by the approach of an excited piece of ſealing-wax, the quality of the electricity may be eaſily aſcertained after the uſual manner.—Put up the lateral frames again, and the electricity will apparently vaniſh;—let them down, and the electricity will re-appear, and ſo on.

If you touch any part of the tin plate or tin tubes with your finger, the electricity is thereby intirely removed, and that will be the caſe whether the lateral frames are up or down.

Exp. II. Take an extended piece of tinfoil, about four yards ſquare, and, holding it by a ſilk thread, electrify it ſo weakly as not to be capable of affecting an electrometer; then bring it in contact with the tin plate of the collector, whilſt the lateral frames are up. This done, remove the tin-foil, let down the lateral frames one after the other, and on doing this, the electrometer W will immediately manifeſt a conſiderable degree of electricity. But if the electrometer were to ſhew no ſenſible degree of electricity, a ſmaller collector, *viz.* one having a tin plate of about four ſquare inches, muſt be brought into contact with the tin plate of the large collector, whilſt the lateral frames of the latter only are down; and then the ſmall collector being removed from the large one, its lateral frames are opened, and its tin plate is preſented to an electrometer, which will thereby be electrified to a much greater degree than the electrometer W was by the large collector.

Exp. III. Let a common cork-ball electrometer be faſtened to an inſulated conductor, having about two or three ſquare feet of ſurface, and communicate to it ſuch a quantity of electricity as may be ſufficient to let the balls of the electrometer ſtand at about one inch aſunder. In this ſtate bring the conductor in contact with the tin plate of the collector for a very ſhort time, and it will be found, that the balls of its electrometer will immediately approach and touch each other, ſhewing that the electricity of the conductor is gone to the plate of the collector; and, in fact, if you let down the lateral frames, the balls of the electrometer W will immediately repel each other to a very great degree.

It ſeems, therefore, to be clearly ſhewn by theſe experiments, that the tin plate of this inſtrument can collect and retain a vaſt quantity of electricity, when the conducting ſurfaces of the lateral frames are contiguous to it, in compariſon to that quantity which it can either collect or retain when thoſe ſurfaces are removed from its vicinity.

The quantity of electricity, which the tin plate ABCD is capable of collecting, principally depends on three circumſtances, *viz.* 1ſt, on the diſtance between the tin plate and the conducting lateral ſurfaces; the ſmaller that diſtance is, the greater being the collecting power; 2dly, on the ſize of the inſtrument; and, 3dly, on the quantity of electricity poſſeſſed by the body from which it muſt be collected or taken away.

I need not expatiate on the principle upon which the action of this inſtrument depends; this being the ſame as that of the electrophorus, of M. VOLTA's condenſer, and of many other electrical experiments; namely, that a body has a much greater capacity for holding electricity when its ſurface is contiguous to a conductor which can eaſily acquire the contrary electricity, than when it ſtands not in that ſituation.

I ſhall laſtly add, that having actually uſed this new inſtrument in ſeveral experiments, I have found it to anſwer perfectly well; one of its principal recommendations being the certainty of its operation.

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XVII. *On the Converſion of a Mixture of dephlogiſticated and phlogiſticated Air into nitrous Acid, by the electric Spark.* By Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read April 17, 1788.

IN Volume LXXV. of the Philoſophical Tranſactions, p. 372. I related an experiment, which ſhewed, that by paſſing repeated electric ſparks through a mixture of atmoſpheric and dephlogiſticated air, confined in a bent glaſs tube by columns of ſoap-lees and quickſilver, the air was converted into nitrous acid, which united to the ſoap-lees and formed nitre. But as this experiment has ſince been tried by ſome perſons of diſtinguiſhed ability in ſuch purſuits without ſucceſs, I thought it right to take ſome meaſures to authenticate the truth of it. For this purpoſe, I requeſted Mr. Gilpin, Clerk of the Royal Society, to repeat the experiment, and deſired ſome of the Gentlemen moſt converſant with theſe ſubjects to be preſent at putting the materials together, and at the examination of the produce.

This laborious experiment Mr. Gilpin was ſo good as to undertake. It was performed in the ſame manner, and with the ſame apparatus, which was uſed in my own experiments, and which is deſcribed in the beginning of the above-mentioned Paper, and is accompanied with a drawing. The method uſed for introducing air into the bent tube, was that deſcribed in the laſt paragraph of p. 373. in that Paper, by means of the apparatus repreſented in fig. 3. or the reſervoir, as I ſhall call it. The ſoap-lees, like thoſe of my own experiments, were prepared from ſalt of tartar, and were of ſuch ſtrength as to yield $\frac{1}{10}$ of their weight of nitre when ſaturated with nitrous acid. The dephlogiſticated air was prepared from turbith mineral, and ſeemed by the nitrous teſt to contain about $\frac{1}{18}$ part of phlogiſticated air.

On December 6, 1787, in the preſence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. J. Hunter, and Mr. Macie, the materials were put together. The quantity of ſoap-lees, introduced into the bent tube, was 180 meaſures, each of which contained one grain of quickſilver; and, as the bore of the tube was rather more than one-third of an inch in diameter, it formed a column of five or ſix-tenths of an inch in length, which, by the introduction of the air, was divided into two parts, one reſting on the quickſilver in one leg of the tube, and the other on that in the other leg. The dephlogiſticated air was mixed with one-third part of its bulk of atmoſpheric air of the room in a ſeparate jar, and the reſervoir was filled with the mixture; and from thence Mr. Gilpin, as occaſion required, forced air into the bent tube, to ſupply the place of that abſorbed by means of the electric ſpark.

From what has been ſaid, it appears, that the mixture employed contained a leſs proportion of common air than that uſed in either of my experiments. This made it neceſſary for Mr. Gilpin now and then to introduce ſome common air by

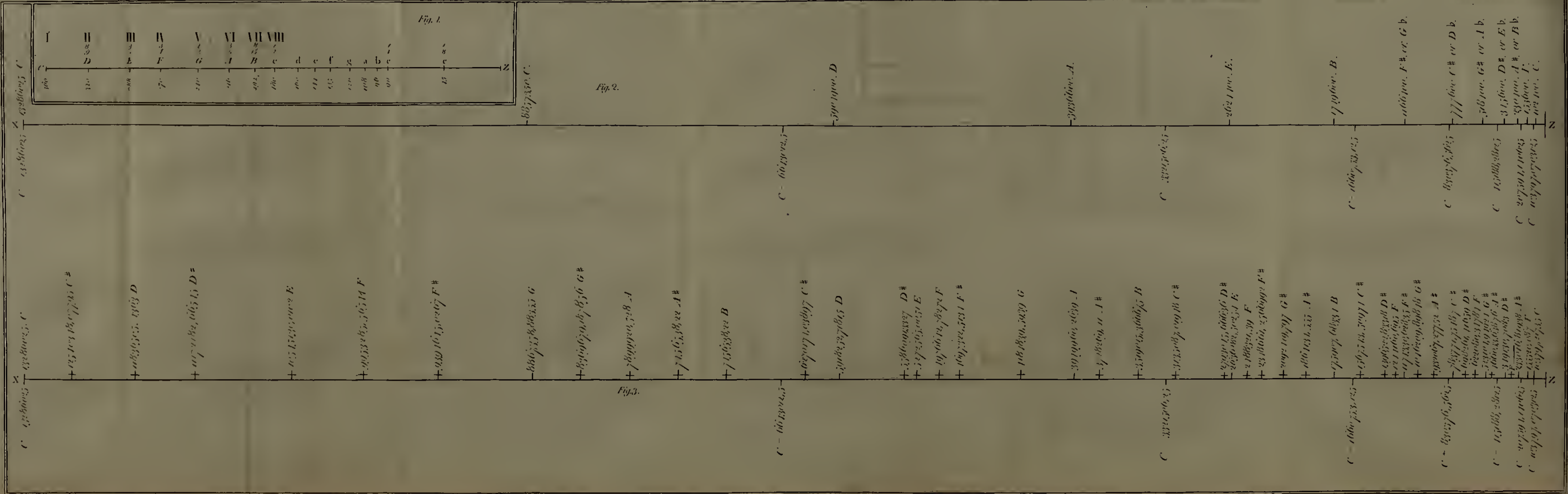

Fig. 1.

Fig. 2.

Fig. 3.

means of the bent tube repreſented in fig. 3. of the abovementioned Paper, whenever from the ſlowneſs of the abſorption he thought there was too ſmall a proportion of phlogiſticated air in the tube.

My reaſon for this manner of proceeding was, that as my firſt experiment ſeemed to ſhew, that the dephlogiſticated air ought to be in a rather greater proportion to the phlogiſticated than the latter did, I was ſomewhat uncertain as to the proper quantities, and doubted whether I could proportion them in ſuch manner as that it ſhould not be neceſſary, during the courſe of the experiment, to add either dephlogiſticated or common air. I therefore mixed the airs in ſuch proportion, that I was ſure there could be no occaſion to add the former; ſince it was much eaſier, as well as more unexceptionable, to add common air than dephlogiſticated air.

On December 24, as the air in the reſervoir was almoſt all uſed, this apparatus was again filled in the preſence of moſt of the above-mentioned Gentlemen, with a mixture of the ſame dephlogiſticated air and common air, in the ſame proportions as before; and the ſame thing was repeated on January 19.

On January 23, the bent tube was, by accident, raiſed out of one of the glaſſes of mercury into which it was inverted, by which it was filled with air, and a good deal of the ſoap-lees were loſt; there, however, was enough remaining for examination.

On January 28, and 29, the produce of this experiment was examined in the preſence of Sir JOSEPH BANKS, Dr. BLAGDEN, Dr. DOLLFUSS, Dr. FORDYCE, Dr. HEBERDEN, Dr. J. HUNTER, Mr. MACIE, and Dr. WATSON. It appeared that 9290 meaſures of the mixed air had been forced into the bent tube from the reſervoir*. Beſides this, Mr. GILPIN had at different times introduced 872 meaſures of common air, which makes in all 10162 of air, conſiſting of 6968 of dephlogiſticated air, and 3194 of common air. But as there were 900 meaſures of air remaining in the tube when the accident happened, the quantity abſorbed was only 9262; but this is a much greater quantity that what from my own experiments ſeemed neceſſary for this quantity of ſoap-lees.

The ſoap-lees were poured into a ſmall glaſs cup, and the tube waſhed with a little diſtilled water, in order that as little as poſſible might be loſt. As they were by this means conſiderably diluted, they were evaporated to dryneſs; but it was difficult to eſtimate the quantity of the ſaline reſiduum, as it was mixed with a few particles of mercury.

Some vitriolic acid, dropped on a little of this reſiduum, yielded a ſmell of nitrous acid, the ſame as when dropped on nitre phlogiſticated by expoſure to the fire in a covered crucible; but it was thought leſs ſtrong. The remainder was diſſolved in a ſmall quantity of diſtilled water, and the following experiments were tried with the ſolution.

It did not at all diſcolour paper tinged with the juice of blue flowers.

It left a nauſeous taſte in the mouth like ſolutions of mercury, and moſt other metallic ſubſtances.

Paper dipped into it, and dried, burnt with ſome appearance of deflagration, but not ſo ſtrongly or uniformly as when dipped in a ſolution of nitre. The marks of deflagration, however, were ſtronger than when the Paper was dipped into a ſolution of mercury in ſpirit of nitre, but not ſo ſtrong as when equal parts of this ſolution and ſolution of nitre were uſed.

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* The method of aſcertaining the quantity of air forced in was by weighing the reſervoir, as mentioned in the above-mentioned Paper, p. 374.

A ſolution of fixed vegetable alkali, dropped into ſome of it diluted, produced a ſlight reddiſh-brown precipitate, which afterwards aſſumed a greeniſh colour.

A bit of bright copper being dipped into it, acquired an evident whitiſh colour, though not ſo white as when dipped into the ſolution of mercury in ſpirit of nitre.

From theſe experiments it appears, that the mixture of the two airs was actually converted into nitrous acid, only the experiment was continued too long, ſo that the quantity of air abſorbed was greater than in my experiments, and the acid produced was ſufficient, not only to ſaturate the ſoap-lees, but alſo to diſſolve ſome of the mercury. The truth of the latter part is proved by the metallic taſte of the reſiduum, its not diſcolouring the blue paper, the precipitate formed by the addition of fixed alkali, and the white colour given to the copper; and the nitrous fumes produced by the addition of oil of vitriol, as well as the manner in which paper impregnated with the reſiduum burnt, ſhew as plainly, that the acid produced was of the nitrous kind. It is remarkable, however, that during this experiment there were no ſigns which ſhewed when the ſoap-lees became ſaturated. The only time when the diminution proceeded much ſlower than uſual was on January 4. It then ſeemed to go on very ſlowly; but as the air abſorbed at that time was only 4830 meaſures, which is much leſs than what ſeems requiſite to ſaturate the alkali, and as the diminution immediately went on again upon adding more common air, it ſeems not likely, that the ſoap-lees were ſaturated at that time.

On January 10, Mr. Gilpin obſerved a ſmall quantity of whitiſh ſediment on the ſurface of the mercury; which ſeems to ſhew, that the ſoap-lees were then ſaturated, and that the acid was beginning to corrode the mercury. The quantity of air abſorbed was alſo 6840 meaſures, which is about as much as I expected would be required. However, as I was perſuaded, from the event of my own experiments, that the diminution would either intirely ceaſe, or go on very ſlowly, as ſoon as the ſoap-lees were ſaturated; and as I was unwilling to ſtop the experiments before that happened, I thought it beſt to continue the electrification.

On the ſame morning Mr. GILPIN found, that about 120 meaſures of the air in the bent tube had been ſpontaneouſly abſorbed during the night, the quantity therein being ſo much leſs than it was the preceding evening, though the electrical machine had not been worked, or any thing done to it during the intermediate time. The reaſon of this in all probability is, that as the acid was then corroding the mercury, the ſoap-lees became impregnated with nitrous air, which, during the night, united to the dephlogiſticated air, and cauſed the diminution.

Though in reality the event of this experiment was ſuch as to eſtabliſh the truth of my poſition, that the mixture of dephlogiſticated and phlogiſticated air is converted by the electric ſpark into nitrous acid, as fully as if the experiment had been ſtopped in proper time; yet, as the event was in ſome meaſure different from that of my own experiments, and might afford room for cavil, I was deſirous of having it repeated; and as Mr. GILPIN was ſo obliging as to undertake it again, the materials were, on February 11. put together for a freſh experiment, in the preſence of moſt of the above-mentioned Gentlemen. The ſoap-lees employed were the ſame as before, but 183 meaſures were now introduced. The dephlogiſtcated air was different, the former parcel being all uſed. It was prepared, like the former, from turbith mineral, but was rather purer, as it ſeemed to contain only $\frac{1}{32}$ of phlogiſticated air. The proportion in which it was mixed with common air was that of 22 to 10; ſo that a greater proportion of common air was now uſed, in conſequence of which it was not neceſſary for Mr. Gilpin to introduce common air ſo often.

On February 29, the reſervoir was again filled with air of the ſame kind, in preſence of ſome of the ſame Gentlemen. As it was found by the laſt experiment that we muſt not depend on the ſaturation of the ſoap-lees being made known by any alteration in the rate of diminution, the proceſs was ſtopped as ſoon as the air abſorbed was ſuch as from my own experiments I judged ſufficient to neutralize the ſoap-lees. This was effected on the 15th of March. The air remaining in the tube, when Mr. Gilpin left off working, was 600 meaſures; but at the time the produce was examined, it was reduced to about 120, ſo much having been abſorbed without the help of any electrification, which is a ſtill more remarkable inſtance of ſpontaneous abſorption than what occurred in the former experiment. A few days after the experiment began, a black film was formed in one of the legs, which, I ſuppoſe, muſt have been a mercurial *ethiops*; but whether owing to ſome ſmall degree of foulneſs in the mercury or tube, or to any other cauſe, I cannot tell. This foulneſs ſeemed not to increaſe; but on March 10, when the air abſorbed was about 5200, a whitiſh ſediment began to appear on the ſurface of the mercury.

On March 19, the produce was examined in the preſence of Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson.

The mixed air forced into the bent tube from the reſervoir was 6650 meaſures, beſides which Mr. GILPIN had at different times introduced 630 of common air, which makes in all 7280, containing 4570 of dephlogiſticated, and 2710 of common air.

The ſoap-lees were evaporated to dryneſs as before. The reſiduum weighed two grains, but there were two or three globules of mercury mixed with it, which might very likely weigh half a grain. This being diſſolved in a ſmall quantity of water, the following experiments were made with it.

It did not at all diſcolour paper tinged with blue flowers.

Slips of paper were dipped into it, and dried; and, by way of compariſon, other ſlips of paper were dipped into a ſolution both of common nitre and phlogiſticated nitre, and alſo dried. The former burnt in the ſame manner, and with as ſtrong marks of deflagration, as the latter.

It had a ſtrong taſte of nitre, but left alſo a ſlight metallic taſte on the tongue.

It did not give any white colour to a piece of clean copper put into it.

In order to ſee whether the whitiſh ſediment, which was before ſaid to be formed in the bent tube, contained any mercury, the remainder of this ſolution was diluted with ſome more diſtilled water, and ſuffered to ſtand till the white ſediment had ſubſided. The clear liquor being then poured off, the remainder, containing the ſediment, which ſeemed to amount only to a very ſmall quantity, was put on a piece of bright copper, and dried upon it; a piece of clean gold was then laid over it, and both were expoſed to heat. Both metals acquired a whitiſh colour, eſpecially the gold, but which was very indeterminate.

Fig. 1

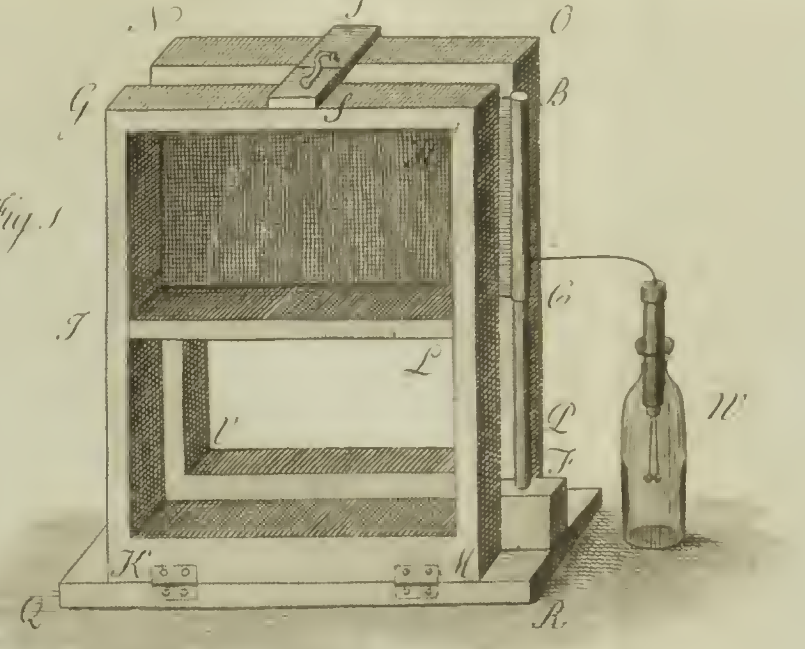
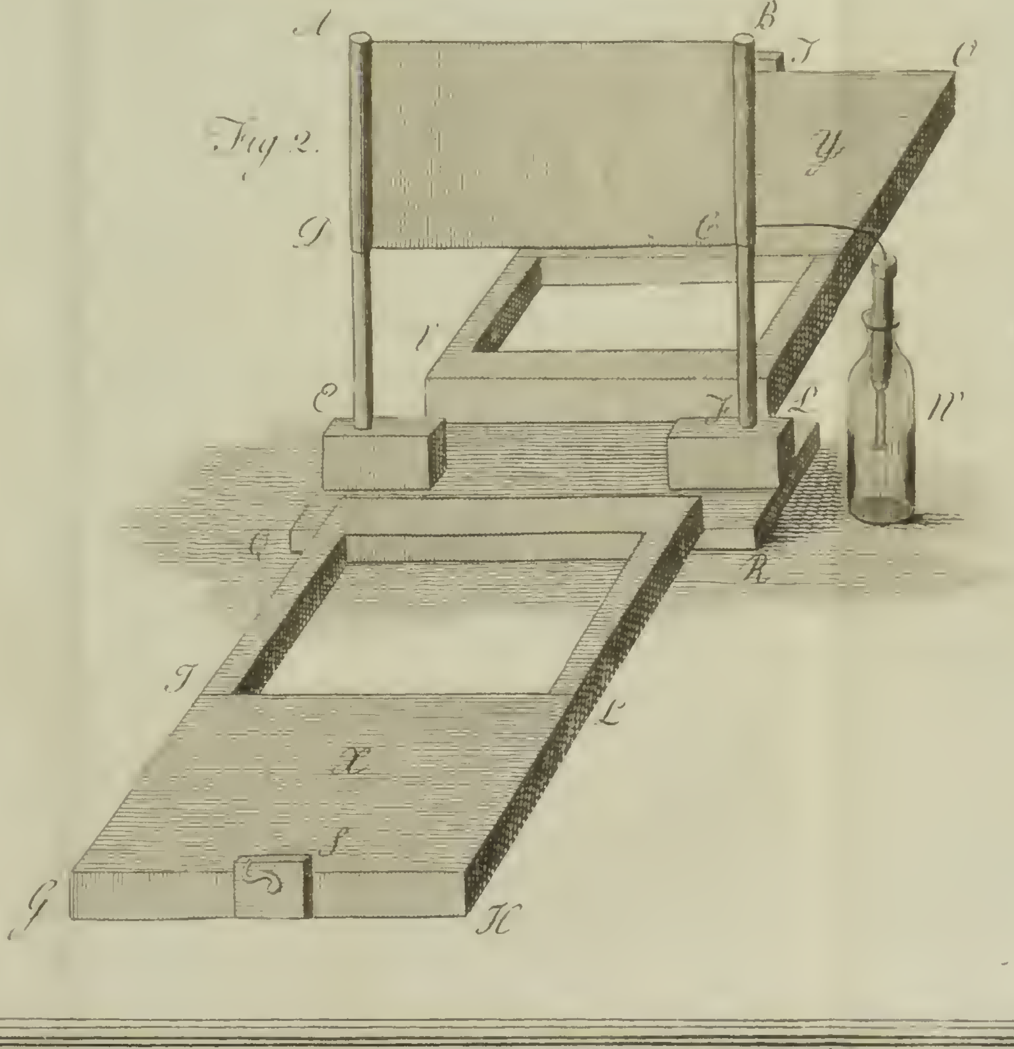


Fig. 2.



Cavallo del.

BRITISH MUSEUM NATURAL HISTORY

In order to diſcover how nice a teſt of alcalinity the paper tinged with blue flowers was, a ſaturated ſolution of common nitre was mixed with $\frac{1}{120}$ of its bulk of the ſoap-lees; and this mixture was found to turn the paper evidently green; ſo that, as the ſolution of nitre contains about twice as much alkali as the ſoap-lees, it appears, that if the reſiduum had wanted only $\frac{1}{240}$ part of being ſaturated, it would have diſcoloured the paper.

From the foregoing trials it appears, that the mixture of dephlogiſticated and common air in this experiment was actually converted into nitrous acid, and was ſufficient not only to ſaturate the ſoap-lees, but alſo to diſſolve ſome of the mercury. The quantity diſſolved, however, was very ſmall, and not ſufficient to diminiſh ſenſibly the deflagrating quality of the nitre; ſo that the proof of the air being converted into nitrous acid was as evident as if no mercury had been diſſolved.

In this experiment, as well as the former, no indication of the ſoap-lees becoming ſaturated was afforded by any ceſſation in the diminution of the air; whereas, in my experiments, it was very manifeſt. I do not know what this difference ſhould be owing to, except to Mr. GILPIN's giving much ſtronger electrical ſparks than I did. In his experiments the metallic knob which received the ſpark, and conveyed it to the bent tube, was uſually placed at about $2\frac{1}{2}$ inches from the conductor, ſo that the ſpark jumped through $2\frac{1}{2}$ inches of air, in paſſing from the conductor to the knob, beſides from $1\frac{1}{2}$ to $2\frac{1}{2}$ inches of air in the tube; whereas in my experiments, I believe, the knob was never placed at the diſtance of more than $1\frac{1}{4}$ inch from the conductor, and the quantity of air in the tube was much leſs; but the conductor and electrical machine were the ſame.

Except this, the only difference I know in the manner of conducting the experiment is, firſt, that Mr. GILPIN uſually continued working the machine for half an hour at a time, whereas I ſeldom worked it more than ten minutes; and, ſecondly, that in Mr. GILPIN's Experiments the common air in the reſervoir bore a leſs proportion to the dephlogiſticated air than in mine; in conſequence of which it was neceſſary for him frequently to introduce common air. On this account, the proportion of the two airs in the bent tube would be conſiderably different at different times; but on the whole, the common air abſorbed bore a greater proportion to the dephlogiſticated than in mine.

Though the whole quantity of air abſorbed in theſe experiments is known with conſiderable preciſion, yet it is impoſſible to determine, with any accuracy, how much of each kind was abſorbed, on account of our uncertainty about the nature of the air which remained at the end of the experiment. But if in the laſt experiment we ſuppoſe that the air abſorbed ſpontaneouſly between the 15th and 19th of March was intirely dephlogiſticated, and that what remained at the end of that time was of the purity of common air, it will appear, that 4090 of dephlogiſticated and 2588 of common air, which is equivalent to 4480 of pure dephlogiſticated air and 2198 of phlogiſticated air, were abſorbed at the time the electrification was ſtopped, and conſequently the dephlogiſticated air is $\frac{204}{100}$ of the phlogiſticated air; whereas in my firſt experiment it ſeemed to be $\frac{220}{100}$, and in my laſt $\frac{253}{100}$.

But the quantity of acid produced, and conſequently, I ſuppoſe, the ſaturation of the ſoap-lees, depends only on the quantity of phlogiſticated air abſorbed; and the effect of the greater or leſs quantity of dephlogiſticated air is only to make the nitre produced more or leſs phlogiſticated. Now, in this experiment, the bulk of the phlogiſticated air was $12\frac{2}{10}$ that of the ſoap-lees. In my firſt experiment it was $11\frac{9}{10}$, and in my laſt $10\frac{8}{10}$.

As many perſons ſeem to have ſuppoſed that the diminution of the air in theſe experiments is much quicker than it really is, though I do not know any thing in my Paper which ſhould lead to ſuppoſe that it was not very ſlow, it may be proper to ſay ſomething on this head. As the quickneſs of the diminution depends ſo much on the power of the electrical machine, I can only ſpeak as to what happens with the machine uſed in theſe experiments. This was one of Mr. NAIRNE's patent machines, the cylinder of which is $12\frac{1}{2}$ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the ſpark was placed at two or three inches from another ball, fixed to the end of the conductor. Now, when the machine worked well, Mr. GILPIN ſuppoſes he got about two or three hundred ſparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 meaſures, but was uſually greateſt when there was moſt air in the tube, provided the quantity was not ſo great as to prevent the ſpark from paſſing readily.

The only perſons I know of, who have endeavoured to repeat this experiment, are, M. VAN MARUM, aſſiſted by M. PAETS VAN TROOTSWYK; M. LAVOISIER, in conjunction with M. HASSENFRATZ; and M. MONGE. I am not acquainted with the method which the three latter Gentlemen employed, and am at a loſs to conceive what could prevent ſuch able philoſophers from ſucceeding, except want of patience. But M. VAN MARUM, in his *Premiere Continuation des Expériences, faites*

faites par le moyen de la Machine électrique Teylerienne, p. 182. has deſcribed the method employed by him and M. VAN TROOTSWYK. They uſed a glaſs tube, the upper end of which was ſtopped by cork, through which an iron wire was paſſed, and ſecured by cement, and the lower end was immerſed into mercury; ſo that the electric ſpark paſſed from the iron wire to the ſoap-lees. After ſo much of a mixture of five parts of dephlogiſticated and three of common air as was equal to twenty-one times the bulk of the ſoap-lees * was abſorbed, ſome paper was moiſtened with the alkali, which by its burning appeared to contain nitre, but ſhewed that the alkali was not near ſaturated. The experiment was then continued with the ſame ſoap-lees till more of the air, equal to fifty-ſix times the bulk of the ſoap-lees, was abſorbed, which is near double the quantity required to ſaturate them; but yet the diminution went on as faſt as ever. It was then tried, by the burning of paper dipped into them, how nearly they were ſaturated; but they ſtill ſeemed far from being ſo.

The circumſtance of uſing the iron wire appears evidently objectionable, on account of the danger of the iron wire being calcined by the electric ſpark, and abſorbing the dephlogiſticated air; and when I firſt read the account, I thought this the moſt probable cauſe of the difference in the reſult of our experiments; but I am now inclined to think that the caſe was otherwiſe. From the manner in which M. VAN MARUM expreſſes himſelf, it ſeems that the only circumſtance, from which they concluded that the alkali was not ſaturated, was the imperfect marks of deflagration, that the paper dipped into it exhibited in burning; which, as we have ſeen, might proceed as well from ſome of the mercury having been diſſolved as from the alkali not being ſaturated. I am much inclined to think, therefore, that, ſo far from the ſoap-lees not having been ſaturated, the quantity of acid produced was in reality much more than ſufficient for this purpoſe, and had diſſolved a good deal of the mercury; for the quantity of air abſorbed favours this opinion, and the phænomena agree well with Mr. GILPIN's firſt experiment, in which this was certainly the caſe; whereas, if the diminution had proceeded chiefly from the dephlogiſticated air being abſorbed by the iron, the tube towards the end of the experiment would have been filled chiefly with phlogiſticated air, which would have made the diminution proceed much ſlower than before; but we are told, that it went on as faſt as ever. It is moſt likely, therefore, that the apparent diſagreement between their experiment and mine proceeded only from their having continued the proceſs too long, and from their not having properly examined the produce.

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* This is rather more than half of that requiſite to ſaturate the ſoap-lees.

M. VAN MARUM then proceeds to ſay: "Surpris de cette "différence de réſultat j'envoyai une deſcription exacte de nos "expériences à M. CAVENDISH, le priant en même tems de m'in"ſtruire s'il pourroit trouver la cauſe de cette différence; et "comme la ſeule différence eſſentielle, par laquelle notre expé"rience différoit de celle de M. CAVENDISH, conſiſtoit en ce que "nous avons employé de l'air pur produit du précipité rouge ou "du minium, au lieu de l'air pur produit de la poudre noire "formée par l'agitation du mercure avec le plomb, dont M. "CAVENDISH ne donne pas la maniere de le produire*, je le

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* The uſing the iron wire formed a material difference in our manner of conducting the experiment, and one which may, perhaps, have had great influence on the reſult; but I do not ſee how the uſing ſome other kind of dephlo-

“ priai de me communiquer de quelle maniere il étoit venu a
“ cet air, parceque je deſirois de répéter l'expérience avec ce
“ même air : mais comme il ne m'a fourni aucune élucidation
“ ſur la cauſe vraiſemblable de la différence du reſultat de nos
“ expériences, et qu'il ne lui a pas plu de me communiquer ſa
“ maniere de produire l'air pur qu'il avoit employé pour les ex-
“ périences, m'écrivant, qu'il s'étoit propoſé d'en parler dans un
“ écrit public, la longueur ennuyante de ces expériences nous
“ a fait prendre la reſolution de différer leur continuation, pour
“ obtenir une parfaite ſaturation de la leſſive, juſqu'à ce que M.
“ Cavendish ait publié ſa maniere de produire l'air pur, dont il
“ s'eſt ſervi, nous contentant pour le preſent d'avoir vu, que
“ l'union du principe d'air pur et de la mofette produit de l'acide
“ nitreux, ſuivant la découverte de M. Cavendish.”

As I ſhould be ſorry to be thought to have refuſed any neceſſary information to a Gentleman who was deſirous to repeat one of my experiments, and who by his ſituation was able to do it with leſs trouble than any one elſe, I hope the Society will indulge me in adding a copy of my anſwer, that they may judge whether this is in any degree a fair repreſentation of it.

“ TO M. VAN MARUM.

“ SIR,

“ I received the honour of your letter, in which you inform
“ me of your ill ſucceſs in trying my experiment on the con-

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dephlogiſticated air, inſtead of that prepared from Dr. Priestley's black powder, can in the leaſt degree form an eſſential difference, as in the ſame paragraph in which I mention my having uſed this kind of air in my firſt experiment, I ſay, that in my ſecond experiment I uſed air prepared from turbith mineral.

" verſion of air into nitrous acid by the electric ſpark. It is " very difficult to gueſs why an experiment does not ſucceed, " unleſs one is preſent and ſees it tried; but if you intend to " repeat the experiment, your beſt way will be to try it with " the ſame kind of apparatus that I deſcribed in that Paper. " If you do ſo, and obſerve the precautions there mentioned, I " flatter myſelf you will find it ſucceed. The apparatus you " uſed ſeems objectionable, on account of the danger of the " iron being corroded by abſorbing the dephlogiſticated air."

" As to the dephlogiſticated air procured from the black " powder formed by agitating mercury mixed with lead, as " it was foreign to the ſubject of the Paper, and as I propoſed " to ſpeak of it in another place, I did not deſcribe my method of procuring it. As far as I can perceive, the ſucceſs " depends intirely on carefully avoiding every thing by which " the powder can abſorb fixed air, or become mixed with particles of an animal or vegetable nature, or any other inflammable matter: for which reaſon care ſhould be taken not to " change the air in the bottle in which the mercury is ſhaken, " by breathing into it, as Dr. PRIESTLEY did, or even by " blowing into it with a bellows, as thereby ſome of the duſt " from the bellows may be blown into it. The method which " I uſed to change the air was, to ſuck it out by means of an " air-pump, through a tube which entered into the bottle, " and did not fill up the mouth ſo cloſe but what air could " enter in from without, to ſupply the place of that drawn " out through the tube.

" I am, &c."

With regard to the main experiment, it was not in my power to give him further information than I did; as I pointed out

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the only circumſtance to which, at that time, I could attribute the difference in our reſults. And with regard to the manner of preparing the dephlogiſticated air from the black powder, I have mentioned all the particulars in which my manner of proceeding differed from Dr. PRIESTLEY's, and have alſo explained on what I imagine the ſucceſs intirely depends; ſo that, I believe, no one at all converſant in this kind of experiments will think that I did not communicate to him my method of procuring that air.

XVIII. *Experiments on the Effect of various Subſtances in lowering the Point of Congelation in Water. By* Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read April 24, 1788.

THE experiments which were neceſſary, in order to determine what effect the admixture of various ſubſtances would produce upon the property of water to be cooled below the freezing point*, naturally led me to a more particular conſideration of the power of ſuch admixtures in making water require a greater degree of cold before it congeals. Many curious queſtions occurred on this ſubject, which could only be anſwered by freſh experiments. Theſe were made nearly in the ſame manner as the preceding; that is, the liquor, whoſe freezing point I meant to try, was put into a glaſs tumbler, to the height of two or three inches above the bottom, and the tumbler was then immerſed in a frigorific mixture of common ſalt and ice or ſnow. As the object was ſeldom to cool the liquor as much as poſſible below its freezing point, the operation required no particular care, but the frigorific mixture was commonly permitted to act with its full force: wherefore, ſome parts of the liquor were often much colder than others; and particularly, near the top, a thermometer would uſually ſtand ſeveral degrees higher than toward the bottom, becauſe the ſurface was conſtantly warmed by the air of the room.

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* See p. 125. of this Volume.

My firſt object of inveſtigation was the ratio according to which equal additions of the ſame ſubſtance depreſs the freezing point. I began with common ſalt; and in order to avoid as much as poſſible a fallacy in the experiments, becauſe the ordinary ſalt is never pure, and often not perfectly neutral, I choſe ſome clean ſalt, in fair cryſtals, which is ſold in London under the name of Borrowſtounneſs pure ſalt. This ſalt I diſſolved in diſtilled water, in various proportions, and found the correſponding points of congelation to be as is expreſſed in the annexed table; where the firſt column indicates the number of parts and decimals of water to one part of the ſalt, and the ſecond column ſhews the freezing point found by the experiment. It appeared clearly, on comparing the proportions of water to ſalt, with the correſponding number of degrees which the freezing point was reduced below 32°, that the effect of the ſalt was nearly in a ſimple ratio; namely, that if the addition of a *tenth* part of ſalt to the water ſunk the freezing point about 11 degrees, or to 21°, it would be depreſſed double that quantity, or to 10° nearly, when a *fifth* part of ſalt was diſſolved in the water. To ſhew, therefore, how far this ſimple proportion is exact, I have added a third column to the table, which is made by ſelecting the loweſt freezing point that was obtained without ambiguity in the experiment, and calculating, by a ſimple inverſe proportion, what all the other points ſhould have been according to that ratio. Thus when a fourth part of its weight of common ſalt was diſſolved in water, I found the freezing point of the liquor to be 4°; therefore, to determine what it ſhould be when only $\frac{1}{32}$ part of ſalt was added to the water, the formula is 32 : 4 :: 28 (the number of degrees that the point 4° is below the freezing point of pure water) : 3½; which ſubtracted from 32° gives

28°½ for the freezing point of that ſolution. All the reſt of the third column of the table is found in the ſame manner, and with very little trouble, becauſe 4 × 28 = 112 is a conſtant number, which being divided by the numbers of the firſt column, the quotient is the number of degrees ſought. In all the experiments, none but diſtilled water was employed.

Common Salt.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
32 : 1	29°	28°½
32 : 1	28 +	28½
24 : 1	27½	27⅓
16 : 1	25¼	25
10 : 1	21½	20¾
7,8 : 1	18½	17⅔
6,2 : 1	13½	14
5 : 1	9½	9½
5,5 : 1	7⅕	7
4 : 1	4	4

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of ſalt in four of water proved to be +4°.

The numbers in the third column of the table come ſo near to thoſe in the ſecond, that moſt likely the ſmall differences between them ought to be aſcribed to errors in the experiment; whence we ſhould conclude, that the ſalt lowers the freezing point in the ſimple inverſe ratio of the proportion which the water bears to it in the ſolution.

In the courſe of the experiments made with the different ſolutions of this ſalt, ſome phænomena occurred which deſerve further attention.

1. This ſolution was in one inſtance cooled 8½, and in ſeveral 5 or 6 degrees, below its freezing point; but in general it

ſhot rather more readily than ſome other ſolutions; which I aſcribed, from the analogy of my former experiments, to its leſs tranſparency. Though the common ſalt did not form a turbid ſolution, yet there was ſomething of a greyiſh caſt, much like what is produced by the mixture of a ſmall portion of clay with pure water; and the caution in conducting the operation, neceſſary to make ſolutions of this ſalt cool a certain number of degrees below their freezing point, ſeemed to be nearly the ſame as would be required with water equally tinged by means of clay.

2. When the ſolution was very ſtrong, nearly ſaturated with the ſalt, cooling it tended ſomewhat to leſſen this greyiſh caſt, to render it rather more limpid: as was particularly the caſe in a ſaturated ſolution which I cooled to 0°. I ſuſpect this effect takes place when the ſolution begins to expand with the cold.

3. In the ſtrong ſolutions, when the proportion of ſalt was as much as one-fifth, before the real congelation came on which reduced the whole to one temperature, ſeveral ſmall ſtellated cryſtals formed in the liquor, which increaſed very ſlowly. Theſe were real ice of the ſalt water; but the cold being in this caſe very little below the freezing point of the ſolution, there was little power to make them ſpread; and ſometimes, when this occurred, I believe, part of the ſolution toward the top was rather above than below the freezing point. Similar phænomena were obſerved, and in a ſtill more remarkable manner, in the ſolutions of ſome other ſalts, always when they were very ſtrong: and it may be given as a general fact, that in ſolutions approaching to ſaturation, though the formation of one glacial cryſtal does, as in pure water, tend to make the whole freeze and come up to an uniform temperature, yet the ſhooting goes on much more ſlowly, as if the

ſalt oppoſed ſome reſiſtance, or at leaſt proved an impediment, to the progreſs of the cryſtallization. By ſtirring the ſolution, however, when theſe ſtellated cryſtals once began to form, they quickly increaſed in bulk, or became more numerous, and the liquor aſſumed its freezing temperature.

4. This ſalt with ſnow, in the manner of frigorific mixtures, produced a cold of – 4°.

The next ſalt which I tried for its effect in lowering the freezing point of water, was nitre. It was part of a large compound cryſtal, or bundle of cryſtals, apparently very pure, ſuch as is uſed in manufacturing the beſt gunpowder. This being mixed with the diſtilled water, in different proportions, the ſolutions froze according to the following table.

Nitre.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
32 : 1	30½	30½
24 : 1	30	30
16 : 1	28¾	29
10 : 1	27	27¼
8 : 1	26	26
7,9 : 1	26½	ſalt depoſited
7 : 1	26½	ſalt depoſited
6,85 : 1	27	much ſalt depoſited.

The third column is calculated from the fifth experiment, in which the freezing point of a ſolution of one part of ſalt in eight of water proved to be 26°.

Nitre is well known to differ from common ſalt in being much more ſoluble in warm than in cold water. Hence it would be nothing remarkable, that the ſolutions being made in water above the freezing point, ſome of the ſalt ſhould, when they exceeded a certain ſtrength, be depoſited before they began to freeze. But a further queſtion occurred here, whether, when a ſolution was cooled below its freezing point, the ſalt would ſtill continue to be depoſited; or whether it would not have parted with all the ſalt it was obliged to let go by the time it came to the degree at which it was to freeze, and would retain the remainder notwithſtanding any ſubſequent cooling. To determine this, I noticed carefully the quantities of ſalt depoſited at the bottom of the tumbler, in compariſon with the cold of the ſolution as ſhewn by the immerſed thermometer; and I found, that in ſome caſes (for inſtance, when the ſalt was to the water only as 1 : 10) the depoſition did not begin till after the ſolution had paſſed its freezing point; and that, when it began earlier, ſtill there was no ſtop at the freezing point, but the quantity continued augmenting as the cold of the ſolution proceeded, and, as far as I could judge, rather in an increaſing ratio. Thus when the ſaturated ſolution was cooled 8 or 10 degrees below its freezing point, which often happened, the collection of nitre at the bottom was very great; and in this manner I could render a ſaturated ſolution of nitre no longer ſaturated when it came to freeze, the deficiency being ſometimes ſo great as to raiſe the point of congelation a degree or more. Hence was aſcertained the unexpected fact, that the lower ſuch ſolutions are cooled, the higher is their freezing point.

The nitre depoſited by the ſolution as it cooled, formed, if the veſſel remained at reſt, ſmall but very white and compact priſmatic or needle-like cryſtals, of conſiderable length, pointing different ways, and at laſt curiouſly interwoven with one another. But if theſe were broken down, or the ſolution was ſtirred with any force, the remaining nitre depoſited itſelf in ſuch minute cryſtals as to have much the appearance of a powder; I ſuppoſe from the deſtruction of the regular ſurfaces upon which it would otherwiſe have continued to form.

Frequently, in the ſtronger ſolutions, there appeared near the bottom and ſide of the tumbler many elegant ſtellated cryſtals, perhaps a quarter of an inch in diameter, all ſeparate, but ſometimes crowding very cloſe upon one another, ſo as to exhibit a ſpectacle of much beauty.

The ice of ſolutions of nitre, eſpecially when it began to thaw, was very different from common ice, having a ſoft woolly appearance, as if of a more tender and looſe texture. Something of the ſame kind was obſervable in the ice of all the other ſolutions, ſufficiently diſtinguiſhing it from any that can be formed of pure water.

All the ſolutions of nitre were remarkably limpid, having no tendency to an opaque or turbid caſt; and accordingly they were very eaſily cooled below the freezing point, and could not but with difficulty be made to ſhoot till they had paſſed it many degrees. In two inſtances they cooled more than 10 degrees; namely, a ſolution of one part of nitre in 24 of water cooled ſlowly to 19°½, and then ſhooting, the thermometer came up to 30°; and another ſolution, in which the nitre was to the water as 1 : 10, cooled rather below 16°, and having produced ſome ſtellated cryſtals, roſe, when the perfect congelation took place, up to 27°.

As, when pure water is cooled below its freezing point, the leaſt particle of ice or ſnow brought into contact with it cauſes an inſtant congelation, I was curious to know whether the ſame effect would be produced when ſalts were diſſolved in the water. Therefore, having one of theſe nitrous ſolutions, whoſe proportions were 8 : 1, I cooled it to 24°, about two degrees below its freezing point, and then, no ſalt being depoſited, I put into it a ſmall bit of ice. The effect of this was not inſtantaneous, as in pure water, though ultimately the ſame; the bit of ice gradually enlarged, and when it was ſtirred about in the liquor, a number of ſtar-like cryſtals formed, which being ſcattered through it ſoon brought it to an uniform temperature of 26°. This ſame ſolution, when cooled in a preceding experiment to 18°, had its freezing point at 27°, from the quantity of nitre that had been depoſited. In all ſolutions, therefore, of ſuch ſalts as are much more ſoluble in hot than in cold water, if it be deſired to find their freezing point when they are loaded with as much of the ſalt as the water can contain at that temperature, the moſt effectual method is to oblige them to ſhoot, as ſoon as they can be made to do ſo, by putting in a ſmall bit of ice or ſnow; for thus the fallacy which might otherwiſe ariſe from the depoſition of ſome of the ſalt will be avoided.

A doubt having been ſuggeſted, whether the contact of a cryſtal of ſalt might not alſo bring on the congelation, that experiment was tried, but it produced no effect. Indeed, the formation of ſaline cryſtals in theſe experiments, the liquor ſtill remaining fluid, was a ſufficient proof to the contrary.

There is another phænomenon which I thought took place in ſolutions of nitre and ſome other ſalts, but could not abſolutely ſatisfy myſelf concerning it; namely, that the depoſition of ſome ſalt haſtened that of the remainder. For inſtance, ſuppoſe that a ſolution of nitre in the proportion of 1 : 8, would retain the whole of its ſalt at its freezing point of 26°; then, if another ſolution were made much ſtronger, it would begin to depoſit its ſalt before it came to 26°; and when cooled to

26°, it would not have juſt loſt the difference between the proportions of 6 : 1 and 8 : 1, but would have depoſited more of the ſalt, ſo that now at 26° it would contain perhaps only a tenth of nitre; as if the formation or exiſtence of ſome cryſtals in the ſolution facilitated the ſeparation of the ſalt, or conſtituted an attractive power counteracting the attraction of the ſalt to the water. It would have required a particular ſeries of experiments to aſcertain this as a fact; but ſeveral obſervations incline me to believe it is ſo; and the conſequences to be drawn from it would be ſufficiently curious, and applicable to many points of theory. Of the ſame nature, perhaps, is another circumſtance already hinted at, that after the ſolution has begun to form cryſtals of nitre, the depoſition proceeds with increaſed rapidity as the cooling goes on. This may, indeed, be owing to the attraction of the water to the ſalt decreaſing in a progreſſive ratio with each degree of cold; but it may alſo depend on the attracting ſurfaces being multiplied as more cryſtals are formed: and ſtirring the liquor ſeems to haſten the progreſs of the depoſition; which may be aſcribed to its bringing freſh portions of the ſolution perpetually to thoſe attracting ſurfaces; for, if there be ſuch an attraction, it can ſcarcely be ſuppoſed to take place at any ſenſible diſtance.

Upon the whole it ſeems evident from the preceding table, that the effect of nitre, like that of common ſalt, is to depreſs the freezing point in the ſimple ratio of its proportion to the water; which will be found univerſally true when allowance is made for the depoſition and other ſources of fallacy already enumerated.

This nitre produced, with ſnow, a cold of between 26° and 27°.

As nitre ſunk the freezing point of water ſo little, namely, but 6 degrees, I had recourſe for the next ſet of experiments to that neutral ſalt which, after ſea ſalt, produces the greateſt cold with ice; which is, the common ſal ammoniac. The different ſolutions of this ſalt in water, being ſubmitted to the action of the frigorific mixtures, froze according to the following table.

Sal ammoniac.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
15,7 : 1	24½	24⅓
10 : 1	20½	20
9,8 : 1	20	19¾
7,9 : 1	16½	16¾
6 : 1	12	12
5 : 1	8	8
4 : 1	4	ſalt depoſited

The third column is calculated from the laſt experiment but one, in which the freezing point of a ſolution of one part of the ſal ammoniac in five of water proved to be 8°.

In this table alſo the numbers of the third colum agree ſufficiently with thoſe of the ſecond to ſhew, that ſal ammoniac, like the two preceding ſalts, depreſſes the freezing point in the ſimple ratio of the proportion in which it is mixed with the water.

The following is the account, as written at the time, of the experiment where the ſalt was to the water as 1 : 5. "This "ſolution cooled down to 2° at bottom, but the frigorific mix-"ture being inſufficient, the top remained at 10°. The tum-"bler was then taken out, and the frigorific mixture being "refreſhed by ſtirring and the addition of new materials, it

“ was replaced, when ice ſoon began to form at the bottom, “ like feathered cryſtals, which however ſhot out very ſlowly. “ But upon ſtirring the ſolution, a vaſt number of ſtellated “ feathered cryſtals preſently formed in it, the thermometer “ roſe immediately to 7°½, and was ſoon got to 8°.” I have tranſcribed theſe remarks, not only becauſe they elucidate ſome of the phænomena mentioned in former experiments, but likewiſe on account of the ſtriking analogy the cryſtals bore to the configurations obſerved in a drop of the ſolution of this ſalt viewed with the microſcope. As the drop dries, great numbers of feathered cryſtals ſhoot out from its edges, and ſome more perfect ſtellated cryſtals are often formed in the middle; of which a pretty exact repreſentation may be ſeen in BAKER’s Employment for the Microſcope (p. 110. Pl. II.). The reſemblance of both kinds of cryſtals, in theſe two different ſituations, is very great; and their particular figure is probably due to the ſame cauſe, a confuſed and conſtrained cryſtallization, occaſioned in the one caſe by the haſty evaporation and want of room in the drop, and in the other caſe by the watery congelation acting upon and embarraſſing the natural tendency of the ſalt.

A ſtronger ſolution of ſal ammoniac, the proportions being 1 : 4, exhibited a ſtill more beautiful appearance; for, beſide the feathered cryſtals at bottom, and the ſtars floating in the middle, a third ſort of cryſtals, moſt reſembling the ſtars, attached themſelves to the ſides of the tumbler, and were ſeen through the glaſs as compoſing a great variety of brilliant *facettes*. In this experiment the ſolution was reduced below 1°, before any congelation began to take place, and a certain quantity of the ſalt fell to the bottom in the form of a white powder, about as much as reduced the liquor to the ſtate of a ſaturated ſolution at the freezing point, which therefore, when the congelation took place, proved to be 4°, inſtead of 2°, the number that would have anſwered to this proportion of ſalt.

It has been a queſtion much conteſted, whether ſaline ſolutions depoſit their ſalt upon freezing. That ſome ſeparation, or a tendency to ſeparation, takes place, many facts concur to prove; and among the reſt ſome phænomena obſerved in the above-mentioned experiments. For inſtance, the ſtellated cryſtals, when firſt formed, were barely ſuſpended in the water, and ſometimes they even gradually ſubſided to the bottom; which ſhews, that they conſiſted of ſalt chiefly, only inviſcated with ice, or at leaſt of an over-proportion of ſalt: for the principal maſs of ice formed in a ſaturated ſolution floats in it like common ice in pure water. Beſides, it was almoſt conſtantly found, that when a cruſt of ice, which had been formed in a ſaline ſolution, began to thaw, a thermometer thruſt into it roſe ſomewhat higher than the point at which it had become ſtationary when the congelation took place; an indication that a leſs proportion of ſalt was preſent in it than had been in the whole ſolution. And if, after any ſolution had ſhot and formed a quantity of ice, the tumbler were ſtill kept in the frigorific mixture, the thermometer immerſed in the liquor gradually ſunk lower and lower, as the congelation proceeded. But theſe very obſervations ſhew, that the ſeparation is far from perfect; as the riſe of the thermometer in the former caſe, or its deſcent in the latter, never exceeded one or two degrees, under a great variation in the quantity of ice in the ſolution.

Sometimes in ſolutions of ſal ammoniac, and ſuch other ſalts as ſeparate by the cooling of the water, a ſort of flocculent ſubſtance is formed, which ſubſides in the water, and thereby is diſtinguiſhed from the proper ice of the ſolution, which it otherwiſe much reſembles in appearance. It is compoſed principally of the depoſited ſalt, in very minute cryſtals like powder, inviſcated and kept together with a little ice.

Though ſal ammoniac forms a tolerably tranſparent ſolution, yet I perceive that in theſe experiments it was in few inſtances cooled much below its freezing point; the moſt was nine degrees. I believe this to have been accidental, from no particular attention being now beſtowed on that object.

The ſal ammoniac, mixed with ſnow, produced a cold of from 4° to 4°½ of FAHRENHEIT's ſcale.

Of all the ſolutions ſubmitted to theſe experiments, there were none more tranſparent and elegant than thoſe made with Rochelle ſalt. The water diſſolved a large proportion of this ſubſtance, and had its freezing point ſunk according to the following table.

Rochelle ſalt.

Proportion of water to the ſalt.	Freezing point by the experiments.	Freezing point by calculation.
10 : 1	29°½	29°¾
5 : 1	27½	27½
4 : 1	26⅓	26½
2,6 : 1	24	23½
2,25 : 1	22½	22¼
2 : 1	21	21
1,6 : 1	24	ſalt depoſited

The third column is calculated from the laſt experiment but one, in which the freezing point of a ſolution of one part of the Rochelle ſalt in two parts of water proved to be 21°.

All the ſolutions of Rochelle ſalt bore to be cooled remarkably well. In one inſtance the liquor ſunk 11½ degrees below its freezing point; namely, the ſolution of one part of the ſalt in five of water, whoſe freezing point proved 27°½, and which cooled to 16° before the cryſtals of ice ſhot. In two other inſtances it ſunk fully nine degrees below its freezing point. And, as already has been mentioned, the ſolutions of this ſalt are remarkably tranſparent.

In trying the greateſt cold to be obtained by mixing Rochelle ſalt with ſnow, I could get the thermometer no lower than 24°.

GLAUBER's ſalt likewiſe was ſubjected to the experiments, but its utmoſt effect in producing cold with ſnow appearing to be only two degrees, this was too ſmall a ſcale for ſettling any thing as to the ratio. A ſolution of it in water, in the proportion of 1 : 5, cooled readily to 31°; but the ſalt was depoſited in great quantities, and often ſo faſt, as to ſtop the cooling of the bottom of the liquor entirely, though the veſſel was immerſed in a ſtrong frigorific mixture. This phænomenon has been obſerved before; but does not take place, to the ſame degree, in the ſolutions of any other ſalt with which I am acquainted, though I evidently perceived ſomething of it in the ſtrong ſolutions both of nitre and of ſal ammoniac. It is exactly the converſe of the cold produced by diſſolving ſalts in water; for as there ſome heat is abſorbed, and becomes latent, by the change of the ſalt from a ſolid to a fluid ſtate, ſo here ſome heat is evolved as the ſalt aſſumes the ſolid cryſtalline form. The effect is ſo much more manifeſt with GLAUBER's ſalt only, I imagine, becauſe the formation of the cryſtals proceeds ſo rapidly; whence the quantity of heat generated equals or exceeds the cold communicated by the freezing mixture. Some odd appearances are produced by this ſudden ſtop of the cooling, and the rapid depoſition of ſalt; for inſtance, a particular ebullition in certain parts of the liquor; but any intelligible deſcription of them would be too minute.

Theſe were all the ſalts with an alkaline baſis which I tried. They all agreed as to the chief object of theſe experiments, namely, to determine how much the freezing point of water would be ſunk by diſſolving them in it in various proportions; which by theſe experiments appears to be, as nearly as could be determined, according to the ſimple ratio of the proportion each ſalt bears to the water.

I now reſolved to try a few ſalts with an earthy and metallic baſis.

The common ſal catharticus amarus of the ſhops was the ſpecimen I uſed of an earthy ſalt. It formed a turbid inelegant ſolution, as if dirty; and with various proportions of water produced the following points of congelation.

Sal catharticus amarus.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
16 : 1	31°	31°
10 : 1	30	30½
4 : 1	28¼	28
3 : 1	26⅘	26⅘
2,4 : 1	25½	25½

The third column is calculated from the laſt experiment, where the freezing point of a ſolution of one part of the ſal catharticus amarus in 2,4 of water proved to be 25°½.

No ſalt was depoſited from the ſtrongeſt of theſe ſolutions; and as that which I uſed was a deliqueſcent ſalt, it muſt probably have been in a vaſt proportion to the water, before any ſuch effect would have taken place. I have ſunk a thermometer with it and ſnow to 7°½; which, according to the proportions in the table, would make more than three parts of the ſalt to two of water. Accordingly, a large quantity of the ſalt was required to the ſnow.

No particular phænomenon was obſerved with this ſalt, except the ſingular configuration of its ice, which aſſumed the form of fungi, or of ſome kinds of lichen, with feathered ſtriæ. The ſolutions were difficult to cool much below their freezing point.

Of the ſalts with a metallic baſis, green vitriol affords one of the moſt tranſparent ſolutions in water. It ſinks the thermometer nearly to 27°½ with ſnow, and reduced the freezing point of water according to the following table.

Green vitriol.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	30¾	31
6 : 1	30¼	30½
4 : 1	29¾	29½
3 : 1	28¾	28¾
2,4 : 1	28	28

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of the green vitriol in 2,4 of water proved to be 28°.

The ice formed by theſe ſolutions aſſumed a foliaceous configuration, with a texture of penniform ſtriæ, in ſome reſpects like the appearance exhibited by a drop evaporating under a microſcope, as delineated by BAKER. Scarcely any ſalt gave the point of congelation ſo regularly in the proportion of the quantities mixed with the water, and none afforded ſolutions which cooled more eaſily and readily below the freezing point. In two inſtances the cooling was more than 11 degrees.

Having found that white vitriol, mixed with ſnow, produced a cold of 20°, melting the ſnow remarkably faſt, I was induced to try the freezing point of its ſolutions. But though it diſſolved very readily in water, yet the liquor it formed was ſo turbid and thick, that little ſatisfaction could be derived from the experiments. The only numbers to be relied upon are the following, which agree ſufficiently with the general reſult.

White vitriol.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	31	31
5 : 1	29$\frac{1}{2}$	30
3 : 1	28$\frac{2}{3}$	28$\frac{2}{3}$

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of the white vitriol in three of water proved to be 28°$\frac{2}{3}$.

Theſe ſolutions cooled very ill, none of them having ſunk much below the freezing point, and the ſtrongeſt, which had a copious ſediment, forming a cruſt of ice at the bottom of the tumbler, before it was reduced at all below the term of congelation.

M. ACHARD, of Berlin, having alledged*, that borax inſtead of raiſing the boiling point of water, like other ſaline ſubſtances, very ſenſibly depreſſes it, I determined, however extraordinary the fact might appear, to try whether it had any peculiar effect on the freezing point. But having made the experiment with nearly a ſaturated ſolution of borax, the thermometer when it congealed was evidently below 32°: I believe about a degree.

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* See CRELL's Chem. Annalen, 1786, Vol I. p. 501.

As a neutral or middle ſalt, which when cryſtallized is always nearly of the ſame nature, and diſſolves in a regular proportion in water, ſeemed likely to afford the moſt ſimple caſe of the effect of extraneous admixtures, it was with ſuch that I began theſe experiments. But having found that with them the ſimple ratio prevailed, I proceeded to try ſubſtances of a more variable nature, and capable of being mixed with water in almoſt any proportion; ſuch as acids, alkalies, and ardent ſpirits. A material difference in the law, which ſeemed to occur in theſe new experiments, renders it proper to defer the account of them till ſome reflections on the preceding facts, with a few additional experiments to which they gave riſe, have been premiſed.

It is now, I believe, univerſally allowed, that in frigorific mixtures the melting of the ſnow or ice is the principal cauſe of the cold produced; all that heat which muſt become latent in order to give water its fluid form being taken from the ſenſible heat of the ingredients. But as, when cryſtallized ſalts are employed for the purpoſe, theſe alſo are reduced to a liquid form, there muſt, from this circumſtance, be ſome additional cold produced, ſuch for inſtance as would be occaſioned by diſſolving the ſame ſalt in water. Suppoſe then that the latent heat of water is 150 degrees, and that ſal ammoniac, in diſſolving to ſaturation, produces ſo much cold as ſinks the whole ſolution about 20°; it is evident, that if this ſalt and ice are mixed together in ſuch proportions as juſt to melt one another, the total cold generated in the operation muſt amount to 170 degrees.

And

And ſo much actually is produced before the whole liquefaction is effected; and yet a mixture of theſe two ſubſtances will ſink the thermometer no lower than to 4° of FAHRENHEIT's ſcale. The conſideration of this apparent difficulty has led to the ſuppoſition, that a certain quantity of *fire* is contained in the cryſtals of the ſalt, which being diſengaged in the ſolution keeps up the mixture to a certain temperature*. But I conceive, that the phænomenon depends ſimply on the gradual liquefaction of the ingredients, a neceſſary conſequence of the cold produced. A ſaturated ſolution of ſal ammoniac freezes itſelf at 4°; therefore, when the mixture is reduced, by the liquefaction of the ingredients, to that temperature, no more of them can melt, becauſe any addition of cold would freeze what is already melted; and if the mixture, under ſuch circumſtances, were placed in an atmoſphere of its own temperature, the ingredients would remain for ever in that ſame ſtate, without any further liquefaction. But in an atmoſphere warmer than 4°, they continue to melt, more or leſs ſlowly, as the heat which is gradually communicated furniſhes what is neceſſary to become latent. This communicated ſenſible heat being immediately converted into latent, the mixture will always be kept down to the ſame temperature as long as there is a ſufficient maſs of unmelted materials; and it can ſink no lower, becauſe then the liquefaction would be ſtopped; conſequently ſuch mixtures muſt preſerve, as they have been found to do, a pretty uniform temperature, ſo as to have been formerly uſed for graduating thermometers. And the whole cold produced, or, to ſpeak properly, the whole of the heat made to diſappear, I preſume to be ultimately equal to the full quantity of latent heat belonging to the diſſolved ice and ſalt.

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* DE LUC's Idées ſur la Météorologie, Tom. I. p. 201.

According to this explanation, the greateſt cold to be produced with any ſalt and ice or ſnow ſhould be that with which a ſaturated ſolution of the ſame ſalt freezes; which was formerly eſtabliſhed by M. DE LUC as a fact, and is in general conformable to the preceding experiments. Thus, when a ſolution of one part of ſal ammoniac in four parts of water was put into the frigorific mixture, ſome of the ſalt was depoſited, and then the ſolution on freezing raiſed the thermometer to 4°, which is exactly the cold produced by a mixture of the ſame ſalt with ſnow. Likewiſe the ſtronger ſolutions of nitre had their freezing point between 26° and 27°; thoſe of green vitriol near 28°; and ſo of moſt of the other ſalts, correſponding with the cold of their frigorific mixtures. This agreement, however, is liable to be modified by ſeveral circumſtances. For inſtance, if the ſalt be of that nature that more of it is held in ſolution in warm than in cold water, it becomes extremely difficult to have the ſolution, at the moment it freezes, exactly loaded with that quantity of ſalt which is moſt advantageous for acting upon the ice or ſnow. Too much may have been depoſited by the cooling previous to the congelation; or ſome reſiſtance having occurred to the cryſtallization of the ſalt, more of it may be retained at the moment of congelation than properly belongs to the ſolution at that temperature. The purity of the ſalts likewiſe, their particular nature as inclined to deliqueſce or to fall down in powder in the air, the rapidity of their action upon the ice or ſnow, muſt neceſſarily be taken into the account, as influencing the degree of cold they can produce.

It is an evident corollary from the above-mentioned agreement, that if the freezing point of the ſolution of a ſalt in water in any known proportion be given, its degree of ſolubility may in general be aſcertained, merely by trying the greateſt cold it will produce with ſnow.

Notwithſtanding this uſual correſpondence between the greateſt quantity of a ſalt that could be diſſolved, and the greateſt degree of cold it would produce, compared with the freezing point of its ſolution, I ſaw reaſon to believe that, with ſome ſalts at leaſt, an irregularity took place in this reſpect. The Rochelle ſalt, mixed with ſnow, did not ſink the thermometer lower than 24°; yet the freezing point of a ſolution of one part of it in two of water appears, by the numbers in the table, to have been 21°. It is remarkable, however, that in the experiment with a ſtill ſtronger ſolution, where ſome of the ſalt was depoſited, the freezing point came down to 24°. Likewiſe the ſolution of white vitriol, whoſe freezing point was $28^{\circ}\frac{2}{3}$, depoſited a copious ſediment, and yet produced with ſnow a cold of 20°. This ſediment did not look like cryſtallized ſalt, but gave a turbid appearance to the whole ſolution. The moſt remarkable circumſtance of this nature, I think, occurred with the purified common ſalt; which, when mixed with ſnow, ſunk the thermometer only to -4°. By experiment I found that one part of this ſalt would diſſolve in about $2^{\circ}\frac{1}{2}$ of water. Now, if the proportions of 1 to 4 give the freezing point at $+4^{\circ}$, as by the table; 1 to $2\frac{1}{2}$ would, upon the general analogy, give it between -12° and -13°; that is, 8 or 9 degrees lower than the greateſt cold the ſalt would produce with ſnow. This circumſtance leads to a ſuſpicion, that however uniform the preceding tables appear, yet in reality ſomething of a decreaſing ratio exiſts, and that each ſubſequent addition of a ſalt does not depreſs the freezing point quite ſo much as the preceding. In thoſe ſalts where the whole quantity of depreſſion is but ſmall, and in the upper part of the ſcale in thoſe which have a greater effect, this progreſſion may be inſenſible, or be incapable of determination by ſuch experiments as theſe, which by their nature do not admit of extreme accuracy; but it may become ſufficiently obſervable toward the laſt additions of ſuch ſalts as are ſoluble in great quantity, and ſuch as alter very much the freezing point. I never ſucceeded to freeze the ſaturated ſolution of this Borrowſtounneſs common ſalt, though it has been cooled ſeveral degrees below – 4° without depoſiting any of its ſalt; if the experiment were made in a cold ſufficient for the purpoſe, it would ſhew whether any irregularity takes place in the action of this ſalt upon ſnow, or whether ſuch a decreaſing ratio does exiſt. Perhaps, however, the whole may depend upon the principle alluded to formerly, that the preſence of ſome ſalt favours the depoſition of more: for when a ſalt is mixed with ſnow, it may in this point of view be conſidered as in the ſame ſtate with a ſaturated ſolution of the ſalt which has ſome of it remaining undiſſolved; and which, therefore, will not contain more than properly belongs to it at the given temperature; and the cold of ſuch a frigorific mixture ſhould be no greater than that of the freezing point of the ſaturated ſolution under thoſe circumſtances.

As it is well known that water, after it has been ſaturated with one ſalt, will take up a certain portion of another ſalt without depoſiting any of the former, I was curious to try what effect the addition of this ſecond ſalt would produce on the freezing point; and particularly whether it would depreſs the freezing point of the ſaturated ſolution the ſame number of degrees that an equal proportion of the ſame ſalt would depreſs the freezing point of water; and whether the ſame ſimple ratio would hold good, or any new law take place. To bring this to the teſt of experiment, I took a ſaturated ſolution of nitre, whoſe freezing point of courſe was between 26° and 27°: and adding to it the purified common ſalt in various proportions, I obtained the following reſults.

Compound ſolution of nitre and common ſalt.

Proportion of water to the nitre.	Proportion of water to the common ſalt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
		°	°	°
A ſaturated ſolution.	30,2 : 1	23½	22¾	¾
	15 : 1	20¾	19	1¾
	10 : 1	17½	15¼	2¼
	7,4 : 1	13½	11½	2
	5 : 1	5¼	4	1¼

The ſolution of nitre was one that had been made ſome time, and contained near one-ſixth part of nitre, conſequently more than the water could keep diſſolved in the temperature of the atmoſphere, which was then not much above 40°. Some of it therefore was depoſited at the bottom. This, however, the addition of a $\frac{1}{32}$ part of common ſalt ſoon enabled the water to take up, and the two ſalts formed together a clear ſolution. In the frigorific mixture, before the congelation took place, ſome ſaline cryſtals were again depoſited, which ſeemed to be chiefly, if not entirely, nitre. In the temperature of the air this depoſit was re-diſſolved; and on attending to the circumſtances more carefully, upon a repetition of the cooling, I found, that no ſalt began to be depoſited till the ſolution was reduced to 23°, when ſome needle-like cryſtals appeared, which conſequently were nitre. The ſolution cooled about a degree more, and then, having let fall much ſalt, partly as cryſtals and partly, by ſtirring, as a powder, it was made to congeal by dropping in a bit of ſnow, and the thermometer roſe to 23°½. When more ſalt was added, to render its proportion to the water as 1 : 15, the nitre ſeemed ſtill more difficultly depoſited in the freezing mixture; and this continued to be the effect of further additions of ſalt, the depoſition of ſaline matter previous to the congelation being always leſs, in proportion as the quantity of common ſalt was greater.

It is evident from the freezing points of this compound ſolution, that the common ſalt depreſſed the freezing point of the ſolution of nitre ſomething leſs than it would have depreſſed the freezing point of water, if added to it in the ſame proportion. To ſhew this more evidently, I have added a fourth and a fifth column to the table: the fourth column is formed by taking the freezing point of the ſaturated ſolution of nitre as 26°½, and then finding how many degrees the quantity of common ſalt added would have depreſſed the freezing point of water; this number of degrees, ſubtracted from the conſtant number 26°½, gives the freezing point by calculation; namely, what it ſhould have been if the ſalt had produced the ſame effect upon the ſolution of nitre as it would upon pure water; and the difference between this and the freezing point found by the experiment gives the numbers in the fifth column. From the table it is apparent, that the deficiency of effect from the ſalt goes on increaſing to the third experiment, after which it decreaſes. Probably ſome particular law takes place, which it would require a great number of experiments to develope; but the decreaſe toward the laſt may in part be owing to the greater quantity of nitre which the water, when it began to be loaded with common ſalt, retained at the time of congelation, and which muſt have its effect in depreſſing the freezing point. The above-mentioned circumſtance ſeems rather contradictory to an opinion which has been entertained, that when one ſalt, added to a ſaturated ſolution of another ſalt, enables it to take up more of the former ſalt, it is only becauſe the water of cryſtallization of the ſecond ſalt really adds to the quantity of the diſſolving fluid.

I next proceeded to try a ſimilar experiment with ſal ammoniac and the purified common ſalt, but with this difference, that neither ſalt ſhould be added to the water in ſuch quantity as to come near the point of ſaturation, ſuſpecting that the diminution of effect obſerved in the foregoing experiments might depend, in part at leaſt, upon this circumſtance. The ſal ammoniac, therefore, was diſſolved in water in the proportion of 1 : 10, and the correſponding point of congelation appeared by experiment to be 20°½, agreeing very well with the table of ſal ammoniac formerly given. To this ſolution was added the purified common ſalt, in proportion to the water as 1 : 15, and then as 1 : 10; the reſulting points of congelation were as is ſhewn in the following table, conſtructed in all reſpects as the immediately preceding.

Compound ſolution of ſal ammoniac and common ſalt.

Proportion of water to the ſal ammoniac.	Proportion of water to the common ſalt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
10 : 1	15 : 1	12°¾	13°	+ ¼
10 : 1	10 : 1	9¼	9⅕	− 1/20

Hence it appears, that in this compound ſolution both ſalts produced, as exactly as the experiments can be expected to ſhew, their full effect in depreſſing the point of congelation. When the ſolutions at length froze, after cooling many degrees below the freezing point, the cryſtals ſhot in a very beautiful manner round the bulb and up the ſtem of the thermometer.

In a compound ſolution of Rochelle and common ſalt there was, however, a deficiency of effect. For the ſolution of Rochelle ſalt in the proportion of one part to four of water, having its freezing point at $26^\circ\frac{1}{3}$; when common ſalt was diſſolved in it, in the proportion of $\frac{1}{16}$th, the freezing point appeared by experiment to be $16^\circ\frac{1}{4}$, whereas by calculation it ſhould have been depreſſed nearly to 15°.

A compoſition of three ſalts was affected as follows:

Compound ſolution of Rochelle ſalt, common ſalt, and ſal ammoniac.

Proportion of water to the Rochelle ſalt.	Proportion of water to the common ſalt.	Proportion of water to the ſal ammoniac.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
9,8 : 1	10 : 1	17 : 1	$13^\circ -$	$11\frac{1}{2}^\circ$	$-1\frac{1}{2}^\circ$

The computation is made thus: Rochelle ſalt, in the proportion of 1 : 9,8, depreſſes the freezing point $2^\circ\frac{1}{4}$; common ſalt, in the proportion of 1 : 10, ſinks it $11^\circ\frac{1}{4}$; and ſal ammoniac, in the proportion of 1 : 17, ſinks it 7°; now $2\frac{1}{4} + 11\frac{1}{4} + 7 = 20\frac{1}{2}$; which, ſubtracted from 32, leaves $11^\circ\frac{1}{2}$ for the computed freezing point of this mixture.

The moment I had found by experiment that the addition of a different ſalt to the ſaturated ſolution of any ſalt would ſtill further depreſs its freezing point, it was obvious to conclude, that greater cold could be produced with ſnow by a mixture of ſalts than by means of either taken ſeparately. I made ſeveral experiments with this view, and found it uniformly the fact, that by adding a certain proportion of a ſalt which had leſs power of producing cold with ſnow, to one which had a greater power, the frigorific effect of the latter was ſenſibly increaſed. Paſſing over examples of leſs conſequence, it will be ſufficient to inſtance common ſalt and ſal ammoniac. The ordinary common ſalt I uſed to mix with ſnow, ſunk the thermometer to − 5°; the ſal ammoniac to + 4°; but when ſome of the latter ſalt was mixed with the former, the compoſition produced with ſnow a cold of − 12°. I did not carry the experiments far enough to determine the proportions which anſwered beſt, but ſaw evidently, that a conſiderable variety in this reſpect occaſioned ſcarcely a perceptible difference: more ſal ammoniac was, however, required, than that proportion of it to the common ſalt, which ought, by computation, to have ſunk a ſaturated ſolution of the latter 7 degrees. On ſeveral occaſions I made uſe of this compoſition to obtain a greater degree of cold than common ſalt alone would produce, and found it a very convenient method.

On this principle it is that impure common ſalt always makes a ſtronger freezing mixture than the pure; it being, in fact, a compoſition of ſalts. I have ſeen three ſalts produce a greater cold than two, but have not carried the experiments far enough to aſcertain the limits of this effect *.

M.

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* The curious experiments, related in the Philoſophical Tranſactions for 1787, p. 282. of producing a great degree of cold by diſſolving a mixture of ſalts in water, depend in part upon the ſame principle, the water being capable of reducing more ſalt from a ſolid to a fluid ſtate when ſeveral kinds are

employed,

M. DE REAUMUR propoſed to try what cold gunpowder would produce with ſnow, as a teſt of its goodneſs, that is, of the purity of the nitre in its compoſition. It is evident, from what has been delivered above, that if any ſaline impurity be mixed with the nitre, a frigorific mixture formed with the compoſition will be lower than 26°½, a greater or leſs number of degrees, according to the nature and quantity of the impurity; and if it be only common ſalt, the proportion of it to the nitre may be thus determined pretty nearly.

As the cold produced by common ſalt with ſnow is − 4° or more, and by ſal ammoniac + 4°, it is difficult to conceive in what manner FAHRENHEIT fixed the zero of his thermometer. All thoſe who have examined the few authentic paſſages to be found in authors upon this ſubject, will be ſenſible how vaguely they are expreſſed, leaving it doubtful whether he uſed common ſalt alone, ſal ammoniac alone, or both mixed together. There is no method of ſinking a thermometer exactly to 0° with theſe ſalts and ſnow, but by means of a certain ſmaller proportion of common ſalt added to the ſal ammoniac; and it would have been an extraordinary chance, that FAHRENHEIT ſhould hit preciſely this proportion often enough to make him rely upon the point ſo found as the commencement of his ſcale; more eſpecially as the proportion

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employed, than it could of one of the kinds only. A remarkable fact of this nature, as I imagine, is recorded ſo early as in the Memoirs of the French Academy of Sciences for 1700, p. 116. "On prend une livre de ſublimé corroſif, et une "livre de ſel ammoniac; on les pulveriſe chacun à part; on mêle enſuite les "deux poudres très exactement; on met le melange dans un matras, verſant "par-deſſus trois chopines de vinaigre diſtillé; on agite bien le tout: et ce me- "lange ſe refroidit ſi fort, qu'on a peine à tenir long-tems le vaiſſeau dans les "mains en été. Il eſt arrivé mème quelque fois à M. HOMBERG, que faiſant ce "melange en grande quantité, la matière s'eſt gelé."

is probably no greater than a ſeventh or a ſixth of common ſalt to the ſal ammoniac. Is it poſſible that FAHRENHEIT, finding a conſiderable difference in his experiments, took the mean between them for his zero, without any reſpect to the different nature of the ſalts with which he operated? It appears, that he was at this time ſo little acquainted with the ſubject, as to conſider his zero as the utmoſt limit of cold.

I come now to certain ſubſtances which, by equal additions, ſeem to depreſs the freezing point of water in an *increaſing ratio*. Theſe, as was mentioned before, are acids, alkalies, and ſpirit of wine. I judge this to be the caſe from the following experiments, though they are by no means ſo full and extenſive as I could have wiſhed to make them, had leiſure permitted.

The ſpecific gravity of the vitriolic acid I employed was 1,837 at 62° of heat; its effect upon the freezing point is ſhewn by the following table.

Vitriolic acid.

Proportion of water to acid.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	24½	22¼
5 : 1	12½	12⅖
4 : 1	7½	7½

This table is conſtructed in the ſame manner as thoſe formerly given of the ſolution of ſimple ſalts; the laſt experiment, where the proportion is 4 : 1, being taken as the ſtandard for computation; and the extreme difference between the calculation and experiment is no leſs than 2°¼, upon a reduction of the freezing point from 24°½ to 7°½. The freezing point, ſet down in the table, is that to which the liquor roſe upon congealing, after having been cooled ſeveral degrees lower; which it is proper to remark, becauſe the ice roſe two or three degrees in thawing.

The nitrous acid I employed was ſmoaking, and had its ſpecific gravity 1,454. It acted on the freezing point according to the following table, which is conſtructed in all reſpects like the preceding.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
16,8 : 1	26°⅓	25°⅔
10 : 1	22 −	21½
7,64 : 1	18	18
5,06 : 1	10½	11
4,26 : 1	7	7

The greateſt difference between the calculation and experiment appears here to be only two-thirds of a degree; but that is more than, I think, can be attributed to inaccuracy. Theſe mixtures cooled remarkably well; that in which the water was to the acid as 7,64 : 1 ſunk down to 6° before it froze. The ice heated about a degree before it was melted.

Spirit of ſalt being a very weak acid, its increaſe of ratio was not perceptible within the limits to which I was confined.

Muriatic acid.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	25	25
5,1 : 1	$18\frac{4}{5}$	$18\frac{1}{2}$
3,05 : 1	$9\frac{1}{3}$	$9\frac{1}{3}$

This table is conſtructed as the foregoing; and the ſpecific gravity of the marine acid was 1,163.

Salt of tartar, ſuch as is uſually ſold in the ſhops, was the vegetable alkali I employed. It did not readily deliqueſce, and conſequently was not very cauſtic: the cold it produced with ſnow was − 12°.

Salt of tartar.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	$27\frac{1}{4}$	$26\frac{1}{2}$
7,5 : 1	$25\frac{1}{2}$	$24\frac{4}{5}$
5 : 1	$22\frac{3}{4}$	$21\frac{1}{5}$
3 : 1	15	14
2,5 : 1	$11\frac{3}{4}$	$10\frac{1}{2}$
2 : 1	5	5

Here the greateſt difference between the calculation and experiment is ſomething more than $1\frac{1}{2}$ degree, in ſinking the freezing point from 22°$\frac{3}{4}$ to 5°; but in the higher freezing points of the table it is leſs, as well as in the lower. Perhaps this irregularity in the experiments is in part to be aſcribed to an impurity in the ſalt of tartar; a turbid appearance, and at length a depoſition, took place in all theſe ſolutions, but principally in the ſtronger, occaſioned probably by tartar of vitriol, with which that ſalt is ſo frequently mixed.

The mineral alkali I tried, which was the cryſtallized ſoda of the ſhops, ſhewed no increaſe of ratio; but the ſcale of its operation was too ſmall for a proper judgement to be formed.

Mineral Alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	30 −	$29\frac{9}{10}$
5 : 1	$27\frac{4}{5}$	$27\frac{4}{5}$

This ſalt would not remain ſuſpended in much greater proportion in the cooled water. I conſider the ſolutions as decreaſing in their freezing point by an equal ratio: poſſibly, if the ſalt of tartar had been cryſtallized, and perfectly ſaturated with fixed air, it would alſo have acted in the manner of a neutral ſalt, and produced no increaſe upon the ratio.

My volatile alkali, being the ſal volatilis ſalis ammoniaci, was tried only in two proportions.

Volatile alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	25	25¼
5,18 : 1	1.9	19

Here alſo there is no appearance of an increaſe of ratio, but rather the contrary.

The ſpecific gravity of the ſpirit of wine I employed was ,829 at 62°. It depreſſed the freezing point according to the following table.

Spirit of wine.

Proportion of water to ſpirit.	Freezing point by experiment.	Freezing point by calculation.
	°	°
8,5 : 1	24½	23¾
5 : 1	17	18
3,66 : 1	12¼	12¾
3 : 1	8½	8⅔
2,5 : 1	4	4

The total difference between the calculation and experiment, upon a reduction of the freezing point from 24°½ to 4°, is ¾ of a degree; but the intermediate points are very irregular, and in an oppoſite ſenſe, as if the ratio were decreaſing. It ſeems more probable, that ſome inaccuracy in the experiments, perhaps owing in part to the evaporation of the ſpirits, ſhould be the occaſion of this, than that there ſhould be ſuch an irregularity in the law.

If any perſon ſhould alledge, that the difference between the obſerved and computed freezing points in the foregoing tables is not ſufficient to eſtabliſh the increaſe of ratio, I can only reply, that it appears to me greater than can reaſonably be aſcribed to error in the experiment; eſpecially as ſimilar experiments with neutral ſalts, not conducted more attentively, agreed ſo well together in pointing out a different law. It muſt be allowed, however, that the experiments do not ſhew any increaſe of ratio, except in the vitriolic and nitrous acids, ſalt of tartar, and, with more ambiguity, in ſpirit of wine: from analogy only I ſuſpect it to take place in the other acids, and in the mineral and volatile alkalies provided they are cauſtic. That a different law from what prevails in the neutral ſalts ſhould take place with theſe ſubſtances, ſeems not ſurpriſing, when it is conſidered how much ſtronger attraction they ſhew for water, and how much leſs limited the proportion is in which they will unite to it: for the ſame reaſons, I ſhould not think it extraordinary, if deliqueſcent ſalts, combined with water, ſhould be found to obſerve the ſame increaſing ratio in depreſſing the freezing point.

As other experiments upon the watery congelation of acids, and in lower degrees of cold, have been given to the public, it would have been deſirable to compare them with mine, and thus to have obtained a greater length of ſcale. But the difficulty of aſcertaining the preciſe ſtrength of different acids is ſo great, and a ſmall uncertainty in this reſpect may occaſion ſo much error, that I conceive little reliance can be placed, with regard to the preſent object, upon any experiments except ſuch as are made by diluting the *ſame* acid with different proportions of water.

I ſhall conclude this Paper with the account of an experiment to determine the effect of ſalt upon the expanſion of water by cold. Pure water begins to ſhew this expanſion about the temperature of 40°, that is, 8 degrees above its freezing point. I put a ſolution of common ſalt, in the proportions of 4,8 parts of water to one of the ſalt, and conſequently whoſe freezing point was 8°$\frac{2}{3}$, into an apparatus I had uſed for other experiments of the ſame kind; and found that the ſolution continued to contract till it was cooled to 17°, but had ſenſibly expanded by the time it was cooled to 15°. Suppoſe the expanſion to have begun at 16°$\frac{2}{3}$, it would be juſt 8 degrees above its new freezing point. Hence we have reaſon to conclude, as far as one experiment goes, that the combination of a ſalt with water has no other effect upon its quality of expanding by cold, than to depreſs the point at which that quality begins to be ſenſible, juſt as much as it depreſſes the point of congelation.

It is probable, that the ſubject of this Paper will appear to ſome minute and trifling. Did it lead to nothing further, that opinion might be juſt. But it is by the accumulation of facts of this kind, and eſpecially ſuch as have been developed by Mr. CAVENDISH in his excellent Papers on the freezing of acids *, that the foundation muſt be laid for a general theory of the laws of coheſion, which may ultimately lead to a knowledge of the ſtructure upon which the intimate properties of bodies depend; in the ſame manner as more accurate obſervations of the different motions of the great maſſes in the univerſe, paved the way to the ſublime diſcoveries of KEPLER and NEWTON.

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* Philoſophical Tranſactions, Vol. LXXVI. p. 241. and p. 166. of this Volume.

XIX. *Additional Experiments and Obſervations relating to the Principle of Acidity, the Decompoſition of Water, and Phlogiſton.* *By* Joſeph Prieſtley, *LL.D. F. R S.* *With Letters to him on the Subject, by Dr.* Withering, *and* James Keir, *Eſq.*

Read May 1, 1788.

WHEN I wrote the Paper, which the Society has done me the honour to order to be printed in their Tranſactions, I had found that the decompoſition of dephlogiſticated and inflammable air, by means of the electric ſpark, produced an *acid liquor*, which Dr. WITHERING found to be the *nitrous*; though I ſhould have obſerved, that he expreſſed ſome doubt whether the liquor did not alſo contain ſome other acid beſides the nitrous.

I have ſince that time been deſirous to aſcertain the *quantity of acid* producible from a given quantity of air; and with this view I gave Mr. KEIR as much of the liquor as I had collected from the decompoſition of about five hundred ounce meaſures of dephlogiſticated air, and the uſual proportion of inflammable air mixed with it. The liquor, he informed me, was 442 grains, of the ſpecific gravity of 1022 (that of water being 1000) and that it contained as much acid as was equivalent to 12.54 grains of concentrated acid of vitriol; which quantity of vitriolic acid is capable of ſaturating as much vegetable fixed alkali as is contained in 22½ grains of dry nitre, or about 23¼ grains of nitre cryſtallized in mean temperature. The ſediment of the ſame liquor he alſo ſuppoſed to contain, at leaſt, as much acid as the liquor itſelf.

That this ſediment contains much acid, ſeems evident from this circumſtance, that, when it is firſt formed, it often emits ſmall bubbles, which riſe to the ſurface of the liquor, and continues to do ſo a conſiderable time. This was more particularly the caſe with the ſediment which I had from the tinned iron tubes. Theſe ſmall bubbles, I imagine, conſiſt of nitrous air (formed from the ſuperabundant acid vapour adhering to the metal and the water in the liquor) becauſe when a phial, half filled with this liquor, had ſtood about a week, the air on the ſurface of it inſtantly, and repeatedly, extinguiſhed a piece of lighted wood that was dipped into it.

From the preceding *data*, given me by Mr. KEIR (and making allowance for the indefinite quantity of water contained in the concentrated acid of vitriol) I am inclined to think, that not much more than one-twentieth part of dephlogiſticated air is the acidifying principle, and that nineteen parts are water.

This, I would however obſerve, relates to air fully ſaturated with water, in conſequence of its having been kept in jars ſtanding in water, ſo that I think it poſſible that the water in the drieſt dephlogiſticated air may not amount to more than nine-tenths of its weight. But I have not aſcertained, by any experiment, how much water any of the kinds of air are capable of holding in a diffuſed ſtate, without being any neceſſary part of their conſtitution.

Though Mr. KEIR found the greateſt part of the acid in the liquor with which I furniſhed him to be the *nitrous*, there were evident ſigns of its containing a ſmall portion of *marine* acid, by its making a precipitation with a ſolution of ſilver in nitrous acid. But this mixture of marine acid, he obſerves, is conſtantly found to accompany the production of nitre in the operations of nature. Whether the different ſubſtances from which the dephlogiſticated air was extracted made any difference in this caſe, I cannot tell; but that which I gave Dr. WITHERING was from minium, and that which Mr. KEIR examined was from mangancſe.

In the notes which I took of the firſt production of this liquor I termed it *blue*, and Dr. WITHERING alſo calls it blue, and once a *greeniſh blue*; but that which I gave Mr. KEIR, and all that I have got ſince, is a decided and deep *green*, which Mr. KEIR thinks to be owing to the phlogiſtication of the nitrous acid.

Thoſe philoſophers who are unwilling to admit the doctrine of phlogiſton, will perhaps ſay, that, in thoſe experiments in which a calx is revived by means of inflammable air, this air joins the dephlogiſticated air that was in the calx; and that, in conſequence of this, the metal, being freed from a foreign ſubſtance, reſumes its proper form and qualities, without having received any addition whatever.

But ſince it appeared in the preceding experiments, in which inflammable air was procured by means of ſteam, that the *metal* did not become a *calx*, except in conſequence of parting with inflammable air (or rather with ſomething which, when united to water, is inflammable air) it cannot be ſuppoſed to recover its metallic form without re-imbibing the ſame thing that it had loſt, which thing may be termed *phlogiſton*.

Conſequently, inflammable air being (in the opinion of thoſe who do not admit the doctrine of phlogiſton) the ſame thing with the ſubſtance from which it was procured, only volatilized, and united to water, it will follow, that any ſubſtance imbibing inflammable air muſt become compounded with that other ſubſtance from which the inflammable air was produced; and therefore, all the inferences recited in the former Paper, which tend to eſtabliſh the doctrine of phlogiſton, muſt be admitted.

It will be aſked, what becomes of the dephlogiſticated air which is certainly expelled from red precipitate when it is heated in inflammable air, and converted into running mercury, if the inflammable air enters into the calx, in order to its becoming a metal? I anſwer, that it unites with a part of the inflammable air, and forms nitrous acid; for the water that is collected in this proceſs is ſtrongly acid, as appears by its turning the juice of turnſole red; but the quantity is ſo ſmall, that it will hardly be poſſible to aſcertain what acid it is. Analogy, however, decides clearly in favour of the nitrous.

It may be ſuppoſed, that in this experiment with *red precipitate*, the acid was that which had not been ſufficiently expelled from that ſubſtance in the proceſs by which it is made. But the reſult was the very ſame when I uſed *precipitate per ſe*; and on this occaſion I uſed a portion of that which I procured from M. Cadet in Paris, of which mention is made in the ſecond Volume of my Experiments, p. 36.

On the other hand, that which is expelled from finery cinder, when it is heated in contact with inflammable air (and thereby becomes *iron)* is *pure water*, without any acid. This I found to be the caſe even when the inflammable air had been got from iron by oil of vitriol. Does not this prove that the iron had imbibed what was properly *water*, and not any one conſtituent part of water only?

That water enters into the conſtitution of every kind of air I ſuppoſed, becauſe it certainly does into that of *inflammable*, *fixed*, and *dephlogiſticated* air, and becauſe none of them can be produced except by proceſſes in which water either certainly is, or may be well ſuppoſed to be, preſent. That *nitrous* air alſo contains water, I have lately found from the iron that is heated in it becoming a proper finery cinder.

At the publication of my laſt Volume of Experiments, I had found, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogiſticated air. Having ſince that time repeated this experiment, and afterwards heated the iron, which was by this means increaſed in weight, in inflammable air, the iron loſt its additional weight, and water was copiouſly produced, as in the ſame proceſs with finery cinder, or, as I ſometimes call it, ſcale of iron.

As nitrous air may be deprived of its water, and become phlogiſticated air by heating iron in it, I find that it undergoes the ſame change by being repeatedly tranſmitted through hot porous earthen tubes, through which I ſome time ago diſcovered that vapour will paſs one way, while the air contiguous to the heated tube will paſs the other.

I firſt tried this proceſs with turnings of iron in the tube, by which means the iron was readily converted into finery cinder; but afterwards I found that the ſame change was produced in the nitrous air by the hot tube only. The two bladders which I made uſe of in this experiment (and by the alternate preſſure of which I made the air contained in them paſs through the hot tube) became *red*, juſt as any bladder does that is filled with nitrous air, and then expoſed to the influence of the atmoſphere till it becomes phlogiſticated air, as may be ſeen in my former experiments. In this manner I now always treat the bladders in which I make experiments on air. It prevents them from putrefying, and gives them a firmneſs of texture ſimilar to tanning.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. I heated five grains of charcoal of copper in eight-ounce meaſures of nitrous air, till it was increaſed to ten-ounce meaſures, and the charcoal had loſt one grain. Examining the air, I found about one-fifth of it to be fixed air, and the remainder phlogiſticated. It ſeems, therefore, that nitrous air conſiſts of water, and ſomething that may be called the baſis of nitrous acid, or that ſubſtance which, when united to dephlogiſticated air, will make nitrous acid; and this ſeems to be pure phlogiſton, ſince it is found, as the preceding experiments ſhew, in the pureſt inflammable air. May we not hence infer, that the nitrous is the ſimpleſt of all the acids, and perhaps the baſis of all the reſt?

It is evident, that more water than enters into the compoſition of nitrous air is neceſſary for the change of it into what I have called *dephlogiſticated nitrous air*, becauſe the contact of iron will not, without water, produce that change in it.

Though fixed air, as I have ſhewn, contains water as well as nitrous air, it cannot be deprived of it, and be decompoſed, by the ſame means; for I have heated iron in it by a burning lens, and have alſo made it paſs repeatedly through a hot earthen tube containing turnings of iron, without producing any change in it.

In the former Paper I ſaid, that manganeſe of itſelf gives only the pureſt dephlogiſticated air by heat; but I have now a quantity which, after giving pure air with a moderate heat, gives air that is more than half fixed air with a greater degree of heat. It is evident, as I have obſerved before, that there are very different kinds of manganeſe.

In theſe Papers I have ſuppoſed, with M. LAVOISIER and others, that the *principle of acidity* is in the dephlogiſticated air only; but as the acid is always formed by the union of this air and the inflammable, it may, perhaps, with equal probability be ſuppoſed to be in either of them, or to be a compound of them both.

Mr. WATT deſires me to mention it as his conjecture, that the nitrous acid is contained in the inflammable air as the acid of vitriol is in ſulphur, the phoſphoric in phoſphorus, &c.; and that the dephlogiſticated air does nothing more than develope the acid. Mr. KEIR, who was led to expect that an acid *muſt* be the reſult of the union of dephlogiſticated and inflammable air, becauſe ſome acid is always the conſequence of its union with other inflammable ſubſtances, thinks that both may be neceſſary ingredients in it. Farther experiments may throw more light on the ſubject.

TO THE REV. DR. PRIESTLEY.

DEAR SIR,

I at length ſubmit to your conſideration the trials which I made upon the liquors you produced, by the firing of *dephlogiſticated* and *inflammable air* in cloſe veſſels; and if you think proper you may tranſmit them to the Royal Society, by way of Appendix to your own Paper upon that Subject.

§ I.

Dec. 25, 1787. LIQUOR produced by burning, in a cloſe *copper* veſſel, *inflammable air* got from IRON by means of the ſteam of water, and *dephlogiſticated air*, expelled by heat only, from MINIUM.

This liquor was blue, and contained a reddiſh brown ſediment.

Exp. 1. *Terra ponderoſa lime water*,

2. *Common lime water*, and

3. *Cauſtic fixed alkali*, cauſed a greeniſh precipitation.

4. *Cauſtic volatile alkali* produced a beautiful deep blue precipitate, which was wholly re-diſſolved by the addition of more volatile alkali.

5. *Phlogiſticated alkali* gave a reddiſh brown precipitate.

6. *Nitre of lead*, no change.

7. *Nitre of ſilver*, a bluiſh precipitate.

8. *Litmus*, its colour firſt turned red, then curdled, and at length nearly deſtroyed.

9. The *filtering paper*, employed to ſeparate the ſediment, was a ſlow burning touch-paper.

§ II.

Dec. 30. LIQUOR produced, by burning in a cloſe *copper* veſſel, *inflammable air*, obtained as in § I. and *dephlogiſticated air*, expelled by heat only, from MANGANESE. This liquor was blue, and contained a reddiſh-brown ſediment.

Exp. 1—9. Similar to thoſe in § I.; and the reſults being in all points the ſame, I determined to mix the liquids together,

ther, the quantities being ſmall, and ſubmit them to further examination.

§ III.

Exp. 1. The united liquids being neutralized by the addition of *cauſtic vegetable alkali*, a beautiful green precipitate ſubſided.

2. This *precipitate* diſſolved entirely in cauſtic volatile alkali, changing to a beautiful blue.

3. The ſuper-natant liquid being ſlowly evaporated to dryneſs, the liquor that eſcaped during this proceſs was nothing but *pure water*.

4. The imperfectly cryſtallized reſiduum (3) efferveſced upon the addition of concentrated vitriolic acid, and being ſubmitted to diſtillation, on the firſt application of heat, an orange-coloured vapour aroſe, which was condenſed in a receiver previouſly wetted with pure water.

5. The diſtilled liquor (4) had the ſmell and the taſte of *nitrous acid*, which inſtantly changed the blue of litmus, and that of violet paper to a red; and it acted upon ſilver.

6. *Terra ponderoſa lime water*, added to it, occaſioned no precipitation.

7. Saturated with vegetable alkali it converted cap paper into touch paper, and formed, upon a ſlow evaporation, cryſtals ſimilar to thoſe of nitre.

§ IV.

Jan. 21, 1788. LIQUOR produced by burning *inflammable air*, obtained as before, and *dephlogiſticated air* from *mercurius precip. ruber*, in a cloſe *tinned iron* veſſel.

This liquor was nearly colourleſs, but contained a large quantity of a light brown ſediment.

Exp. 1. 2. 3. 4. 5. 8. 9. Similar to thoſe of § 1. and 11. except the different colour of the precipitates, and that made by the phlogiſticated alkali being a fine blue.

10. The liquor ſaturated with vegetable alkali being ſlowly evaporated to dryneſs, and the precipitates mixed with the brown ſediment being put into a retort, ſome concentrated vitriolic acid was added, and heat was applied. A *very* faint ſmell of nitrous acid aroſe, but no viſible red vapour; nor did any thing diſtil into the receiver until the heat was very conſiderable.

11. The liquor then diſtilled was evidently acid; but this acid was wholly precipitated by terra ponderoſa lime water.

§ V.

Feb. 19. Having now received a larger quantity of the *liquor* and ſediment, produced as § 1. *viz.* 329 grains of the bluiſh green liquor, and 12 grains of the reddiſh brown ſediment, I proceeded to repeat the experiments, but the reſults correſponding with the former ones, I need only mention, that the liquor changed litmus red, and coagulated it; that with phlogiſticated alkali it gave a browniſh red, and with cauſtic vegetable alkali a blue precipitate; that after ſaturation with the vegetable alkali, both the dried ſalt and the ſediment being ſubmitted to diſtillation with the addition of concentrated vitriolic acid, an orange-coloured vapour inſtantly aroſe, and the peculiar ſmell of nitrous acid was ſtrongly perceived. The diſtilled liquor, beſides having the taſte of nitrous acid, diſſolved ſilver; and when neutralized by vegetable alkali and ſlowly evaporated, afforded well-formed cryſtals of common nitre.

Theſe, Sir, are all the trials I have made with the liquors produced in your experiments. They pretty clearly prove the acid generated to be the ſame, whether the dephlogiſticated air was procured from red precipitate of mercury, from minium, or from manganeſe, and that this acid is the *nitrous acid.* It is not quite ſo clear why the liquor and ſediment in § IV. gave no ſtronger marks of the preſence of nitrous acid; but it is evident, that the acid had united itſelf to the iron, if not to the tin, of the veſſel employed: and I find, that when nitrous acid is fully ſaturated with iron by being boiled with it, and fixed alkali is added, this mixture ſubmitted to diſtillation, with the addition of concentrated vitriolic acid, yields no red vapours, and very little ſmell of nitrous acid.

I remain, &c.

W. WITHERING.

Birmingham,
February 22, 1788.

TO THE REV. DR. PRIESTLEY.

DEAR SIR,

I HAVE examined the green liquor which I received from you, with a view to determine the two points you wiſhed principally to know, namely, the *quantity* and the *kind* of the contained acid.

The quantity of the green liquor was 442 grains, and its ſpecific gravity was to that of water as 1022 to 1000. When the green liquor was poured out of the phial, there remained at the bottom 72 grains of a brown powder, which upon examination was found to be a calx of copper, ſoluble in acids, but not in water.

The colour of the liquor ſhewed that it was a ſolution of copper. However, it gave no cupreous precipitation to poliſhed ſteel, till a few drops of acid had been added. This circumſtance ſeemed to ſhew the perfect ſaturation of the acid of the green liquor with the copper; and this complete ſaturation was further evinced by the very ſmall alteration of colour which litmus ſuffered on being mixed with this liquor. This teſt did indeed ſeem to acquire a little tendency to redneſs, but the effect was ſo minute as not to deſerve conſideration, and might probably be owing to ſome ſlight phlogiſtication, as it is called, of the acid contained, rather than to its ſuperabundance.

Upon evaporating the green liquor by expoſure to air, without the application of heat, no cryſtals were formed, but a dry green powder, inſoluble in water, but ſoluble in acids. You had before acquainted me with this fact. As ſolutions of copper in the nitrous acid are known to yield deliqueſcent cryſtals, and as no cryſtallization happened in this inſtance, although the acid was evidently nitrous, I was deſirous of diſcovering the cauſe of this difference between this liquor and common ſolutions of copper in the nitrous acid. Upon making ſome trials with this view, I found that there were three periods or ſtages to be diſtinguiſhed in the combination of copper with the nitrous acid. The firſt period is when the acid is ſuperabundant, and deliqueſcent cryſtals formed. The ſecond period is when the acid has been completely ſaturated, or perhaps ſuperſaturated, by repeated and alternate evaporation to dryneſs, and re-diſſolution in water, and then no cryſtals but a green powder is formed. The third period is when, by a further evaporation of acid and increaſe of heat, the green powder is changed into a brown or black calx. The complete ſaturation of the green liquor ſeems then to be the cauſe of the formation of the green powder on evaporation, inſtead of deliqueſcent cryſtals; and the production of the brown calx may be ſuppoſed to be either the reſult of a calcination of a part of the ſolution of copper which may have taken place in conſequence of the great heat excited by the inflammation of the two airs; or to have been formed by the action of the heated dephlogiſticated air on the copper.

In order to find the quantity of acid that was contained in the green liquor, I added, by ſlow degrees, to 100 grains of this liquor, a ſolution of mild fixed alkali, till no more precipitation took place, taking great care to ceaſe adding alkali as ſoon as the copper ceaſed to precipitate, to prevent a re-diſſolution of the precipitated metal. Upon examining how much of this alkaline ſolution was required to ſaturate a given quantity of oil of vitriol of the common ſpecific gravity, *viz.* to water as 1844 to 1000, I found that the 100 grains of green liquor required for its precipitation as much alkali as would ſaturate 2,837 grains of the above-mentioned acid of vitriol. And as the whole quantity of green liquor was 442 grains, its precipitation would require as much alkali as would be capable of ſaturating 12,54 grains of the concentrated vitriolic acid.

This quantity of vitriolic acid is capable of ſaturating as much vegetable fixed alkali, as is contained in 22½ grains of dry nitre, or about 23¼ grains of nitre cryſtallized in mean temperature *.

The difficulty of finding even an approximation of the quantity of acid, or of air expended in forming the brown calx, muſt be very great; and this circumſtance ſhews the diſadvantage of uſing metallic veſſels, although in other reſpects more commodious for your large experiments than thoſe of glaſs. I think it is probable, that the quantity of acid expended in this manner is as much or more than the quantity retained in the ſolution; but I am ſorry I am not able to give you any grounds for a true determination. However, the experiment which I made with the calx was as follows. I diſſolved it in vitriolic acid, and having ſaturated with alkali the ſuperfluous acid, I precipitated the ſolution by a fixed alkali of known ſtrength, and I found that the quantity of alkali requiſite for this precipitation was as much as would ſaturate ten grains of the oil of vitriol before mentioned. This quantity of vitriolic acid is ſo much leſs than I ſhould expect, that I regret I have not an opportunity of repeating the experiment.

The next point to be determined is the *ſpecies* of acid that is contained in the green liquor. The acid produced in your former experiments of burning inflammable and dephlogiſticated airs was determined to be the nitrous; and every trial which I have made of this green liquor has convinced me, that the ſame acid is contained in it alſo. Upon adding to this liquor ſome vitriolic acid and diſtilled water, I obtained by diſtillation a colourleſs acid liquor, which from the ſmell, the properties of diſſolving ſilver very readily, and of giving a deflagrating quality to paper ſoaked in this liquor when ſaturated with alkali, evidently manifeſted the preſence of the nitrous acid. But beſides the nitrous, which is undoubtedly the acid in greateſt abundance, I think I may venture to affirm the exiſtence alſo of the marine acid.

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* I ſay the *mean temperature*, becauſe the proportion of water retained by ſalts varies very conſiderably, according to the temperature in which they are cryſtallized; the warmer the weather, the leſs water being retained. The conſideration of this fact ought to enter into the eſtimates of the proportions of the component parts of ſalts.

Upon adding to the green liquor a ſolution of ſilver in which the acid was ſuperabundant, the mixture became turbid; and when it was boiled, the minute particles collected and fell to the bottom in form of white flocks, which, on expoſure to light, became dark coloured, and were re-diſſolved by volatile alkali; all which appearances are indications of a luna cornea. Although theſe appearances left no doubt in my mind of the exiſtence of a marine acid, I am nevertheleſs much obliged to our ingenious philoſophical friend Mr. JOHNSON for reminding us, that Mr. CAVENDISH had alſo obſerved, in the acid produced in his experiment of paſſing the electric ſpark through a mixture of dephlogiſticated and phlogiſticated airs, a precipitation upon adding a ſolution of ſilver; from which, however, that moſt accurate philoſopher would not infer the preſence of marine acid, having diſcovered that the precipitation might be produced by the nitrous acid itſelf, when that acid is much phlogiſticated. The acid of the green liquor is in ſome degree phlogiſticated, as appears both from the ſmell which I perceived on adding the ſolution of ſilver, which, as I have mentioned, contained a ſuperabundant acid, and from the green colour of the liquor; for I have never been able to make any but blue or greeniſh-blue ſaturated ſolutions from copper and nitrous acid, although they ſometimes appear green from a ſuperabundant quantity of yellow acid, or from the yellow fumes during the ſolution. By adding a ſolution of melted nitre to the blue ſolutions of copper either in the nitrous or vitriolic acids, the phlogiſticated acid of the melted nitre immediately communicates a green colour. The marine acid tends more than the other acids to give a green colour; but I am inclined to impute the colour of your liquor principally to the phlogiſtication of the acid, becauſe upon adding a very ſmall portion of vitriolic, marine, or nitrous acid, the colour was immediately changed to a light blue. The ſolution of ſilver, being purpoſely acid, always produced this change of colour, and I therefore conſidered, that the phlogiſticated acid was expelled in the operation, and could not affect the precipitation.

Experiments have taught me, that an exceedingly ſlight degree of that quality which is called the phlogiſtication of nitre, is ſufficient to produce an alteration in the colour of ſolutions of copper, and that the colour will be changed when the degree of phlogiſtication is incapable of producing any precipitation of ſilver. Not only a ſolution of melted nitre will always change the colour of cupreous ſolutions, but even cryſtals of nitre, when mixed with the nitrous acid, and the mixture boiled to dryneſs upon copper, and re-diſſolved in water, will give a green ſolution. If cryſtals of nitre be diſſolved in a colourleſs ſolution of ſilver in nitrous acid, and this mixture added to a ſolution of copper in the ſame acid, the blue colour of the latter will be changed to a green, although neither the nitre nor ſolution of ſilver will ſeparately have this effect, neither will nitre diſſolved in a dilute nitrous acid. I do not know what degree of phlogiſtication may be required to precipitate ſilver, but I ſuppoſe it to be very great; at leaſt it is certain, that the degree of phlogiſtication capable of giving a green colour to ſolutions of copper is inſufficient to precipitate ſilver. Mr. CAVENDISH, when he relates the fact of the precipitation of ſilver, ſpeaks only of nitre, *much* phlogiſticated, poſſeſſing this property.

To obviate any ſuſpicion of the precipitation of ſilver being owing to the phlogiſtication of the acid, I added to ſome of the green liquor about an equal quantity of colourleſs nitrous acid and ſome diſtilled water, and I kept this mixture boiling half an hour, in order to diſengage and expel any phlogiſticated acid which it might contain. To the liquor, which remained perfectly pellucid, I added a ſolution of ſilver, upon which the precipitation immediately took place with all its uſual circumſtances, and in as great a degree as before.

If, upon examining the acids which you or others may hereafter obtain by the inflammation of airs, a mixture of marine acid be conſtantly found to accompany the production of the nitrous, the fact will be only analogous to all the other known productions of nitrous acid; in all which, either in the natural formation of nitre as in Spain and India, or in the nitre beds and walls made by art, a very large proportion of marine ſalts is conſtantly obſerved to accompany the nitre.

I ſhall conclude my letter with mentioning a fact, which I obſerved in making ſome experiments to find the cauſe of the formation of the green powder above-mentioned, and which, though not relative to the preſent ſubject, may be thought not unworthy of communication. It is the *decompoſition of common ſalt and ſeparation of its alkali by copper*. As I conceived the opinion that the formation of this green powder by evaporation of this green liquor was owing to the perfect ſaturation of the acid, among other modes of obtaining a perfectly ſaturated ſolution of copper, I ſprinkled upon plates of copper ſtrong ſolutions of common ſalt and of ſal ammoniac. In a few days the ſal ammoniac was found to have converted the copper into a dry green calx; but the common ſalt acted more ſlowly upon the copper, and continued moiſt during ſeveral weeks. At length, however, at the end of two months, a dry green calx was formed, and I obſerved, that round the edges of the calx where it joined to the ſurface of the copper not acted upon, and where the liquor had laſt ſtood before it was dried, an efflореſcence had formed of mild mineral alkali, which ſeems to have been ſeparated from its acid by the action of the copper on the acid, and perhaps aided by the affinity of the alkali itſelf to fixed air.

The ſal ammoniac alſo had probably been decompoſed by the copper, but the volatility of its alkaline baſis, when diſengaged, had prevented its appearance.

Wiſhing you may long continue to add to the numerous and ſplendid liſt of diſcoveries with which you have already enriched natural philoſophy,

I am, &c.

JAMES KEIR,

March 26, 1788.

XX. *On the Probabilities of Survivorſhips between Two Perſons of any given Ages, and the Method of determining the Values of Reverſions depending on thoſe Survivorſhips. By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *D. D. F. R. S.*

Read May 8, 1788.

THE hypotheſis of an equal decrement of life, adopted by M. DE MOIVRE, for the purpoſe of facilitating the computations of life annuities, has not only been rendered unneceſſary by the late publication of many excellent tables deduced from real obſervations [*], but has likewiſe been found ſo very incorrect in ſome caſes, that probably little or no recourſe will ever be had to it in future. But though the direct application of this hypotheſis may be laid aſide, there is danger of its not being entirely abandoned; and mathematicians may ſtill be led to reaſon from this principle, by deriving their rules from the *expectations* rather than from the *real probabilities* of life. The ingenious Mr. THOMAS SIMPSON has contented himſelf with this inaccurate method in his Select Exerciſes, and he has been followed in it by moſt other writers on the ſubject [†]. Even in thoſe caſes which involve only two lives, the errors are often conſiderable, eſpecially if the expectations are derived from the *London*, the *Sweden*, or any other tables, in which the decrements of life are unequal. But when three lives are involved in the queſtion, theſe errors are generally enormous; nor is it ever ſafe, when the ages of thoſe lives differ very much, to have recourſe to rules which are founded upon this principle. The three following problems, though the moſt common in the doctrine of ſurvivorſhips, have never hitherto been ſolved in a manner ſtrictly true. The ſecond of them is of particular importance, and I have taken much pains to examine how far Mr. SIMPSON's ſolution of it may be depended upon. It has, indeed, been ſolved by M. DE MOIVRE [*], and Mr. DODSON [†]: but the firſt of theſe writers has erred moſt egregiouſly in the ſolution itſelf, and the other having derived his rule from a wrong hypotheſis, has rendered it of no uſe. It is much to be wiſhed that the ſolutions of all caſes in reverſions and ſurvivorſhips were deduced, like the three following ones, from the real probabilities of life. Moſt of thoſe which are now in uſe are at beſt but approximations, and can never be relied on with any tolerable degree of ſatisfaction.

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[*] See Dr. PRICE's Treatiſe on Reverſionary Payments, 4th edit.; and Mr. Baron MASERES on Life Annuities.

[†] Dr. PRICE's ſolutions of his 15th and 16th queſtions are exceptions to this remark.

PROBLEM I.

Suppoſing the ages of two perſons, A and B, to be given; to determine the probabilities of ſurvivorſhip between them from any table of obſervations.

SOLUTION.

Let a repreſent the number of perſons living in the table at the age of A the younger of the two lives. Let a', a'', a''',

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[*] See Mr. DE MOIVRE's 17th problem, and Dr. PRICE's remarks upon it in his Treatiſe on Reverſionary Payments, Eſſay 3. Vol. I.

[†] See DODSON's Mathematical Repoſitory, Prob. 23. Vol. III.

a'''', &c. repreſent the decrements of life at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of A. Let b repreſent the number of perſons living at the age of B the older of the two lives, and c, d, e, f, &c. the number of perſons living at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of B. Suppoſing now it were required to determine the probability of B's ſurviving A in the firſt year. It is manifeſt that this event may take place either by A's dying before the end of the year and B's ſurviving that period, or by the extinction of both the lives, reſtrained however to the contingency of B's having died *laſt*. The probability that A dies in the firſt year, and that B ſurvives it, is expreſſed by the fraction $\frac{a'c}{ab}$. The probability that both the lives die in this year is expreſſed by the fraction $\frac{a'\,.\,\overline{b-c}}{ab}$; and as it is very nearly an equal chance that A dies *firſt*, this fraction ſhould be reduced *one-half*, and then it will become $=\frac{a'\,.\,\overline{b-c}}{2ab}$. Hence the whole probability of B's ſurviving A in the firſt year will be $=\frac{a'c}{ab}+\frac{a'\,.\,\overline{b-c}}{2ab}=\frac{a'\,.\,\overline{b+c}}{2ab}$. In the ſame manner the probability of B's ſurving A in the 2d, 3d, 4th, &c. years may be found $=\frac{a''\,.\,\overline{c+d}}{2ab}\ldots\frac{a'''\,.\,\overline{d+e}}{2ab}\ldots\frac{a''''\,.\,\overline{e+f}}{2ab}$, &c. reſpectively; therefore, the whole probability of B's ſurviving A will be $=\frac{1}{ab}\times\overline{\frac{\overline{b+c}}{2}a'+\frac{\overline{c+d}}{2}a''+\frac{\overline{a+e}}{2}a'''+\frac{\overline{e+f}}{2}a''''}$, &c.

Having found by the preceding ſeries the probability of B, the elder life's, ſurviving A the younger; the other expreſſion, which denotes the probability of A's ſurviving B, is well known to be the difference between the foregoing ſeries and *unity*.

The ſum of this ſeries might eaſily be determined from tables of the expectations of ſingle and joint lives*. But no ſuch table as the latter having ever been computed, it will by no means be found a laborious undertaking to compute a table of the probabilities of ſurvivorſhip between two perſons of all ages immediately from this ſeries, without having recourſe to the expectations of life. For if the probability of ſurvivorſhip between any two perſons be found, the probability between two perſons one year younger is obtained with little difficulty; and by proceeding in this manner a whole table may be formed in leſs time than would be neceſſary for computing one of the expectations of two joint lives. To exemplify what I have ſaid, I ſhall juſt ſet down a few operations for determining the probability of ſurvivorſhip, according to the Northampton Table of Obſervations †, between two perſons, whoſe common difference of age is 10 years.

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* The above ſeries, expreſſing the probability of ſurvivorſhip, may be eaſily found, from the ſolution of the next problem, $= \frac{\beta \,.\, \overline{K - AK} - c \,.\, \overline{C - AC}}{2b}$; K denoting the expectation of one life one year younger than B; C the expectation of a life one year older than B; AK and AC the expectations of the joint lives of A and K, and of A and C, reſpectively; and β, as in that problem, the number of perſons living in the table of obſervations at the age of K.

† See Dr. PRICE's Treatiſe on Reverſionary Payments, Tab. 6. Vol. II.

Age of B.	Age of A.	Probability of B's ſurviving A.	Probability of A's ſurviving B.
96	86	$\frac{1}{1\times145}\times\overline{\frac{0+1}{2}\times34}=.1173$ . .	$1-.1173=.8887$
95	85	$\frac{1}{4\times186}\times\overline{\frac{4+1}{2}\times41+17}=.1606$	$1-.1606=.8394$
94	84	$\frac{1}{9\times234}\times\overline{\frac{9\times4}{2}\times48+119.5}=.2049$	$1-.2049=.7951$
93	83	$\frac{1}{16\times289}\times\overline{\frac{16+9}{2}\times55+431.5}=.2420$	$1-.2420=.7580$
92	82	$\frac{1}{24\times346}\times\overline{\frac{24+16}{2}\times57+1119}=.2720$	$1-.2720=.7280$
91	81	$\frac{1}{34\times406}\times\overline{\frac{34+24}{2}\times60+2259}=.2897$	$1-.2897=.7103$
90	80	$\frac{1}{46\times469}\times\overline{\frac{46+34}{2}\times63+3999}=.3022$	$1-.3022=.6978$

It may eaſily be ſeen from theſe ſpecimens in what manner the probabilities of ſurvivorſhip between two younger lives are deduced from the probabilities between two older lives, provided their common difference of age be the ſame; for the numbers 17 . . 119.5 . . . 431.5, &c. in the 2d, 3d, 4th, &c. ſeries are the ſums of the ſeries next preceding. Thus 17 is $=34\times\frac{1}{2}$ . . . 119.5 is $=41\times\frac{5}{2}+17$ . . . . 431.5 is $=48\times\frac{13}{2}+119.5$, &c. It may be neceſſary to obſerve further, that if the ages of the two perſons be equal, the probability of ſurvivorſhip between them being likewiſe equal, is expreſſed by the fraction $\frac{1}{2}$; and that this affords an inſtance of the accuracy of the foregoing inveſtigation; for the ſeries expreſſing the probability in this caſe is the ſame with this fraction, the chance of ſurvivorſhip becoming then (ſince $a=b$; $a'=b-c$; $a''=c-d$, &c.; and $\overline{b+c}\times a'=\overline{b+c}\times\overline{b-c}=b^2-c^2$, &c.) $=\frac{bb-cc}{2bb}+\frac{cc-dd}{2bb}$, &c. $=\frac{1}{2}$.

Mr. SIMPSON, in his Treatiſe on Annuities and Reverſions * has given a curve whoſe area determines the probability of ſurvivorſhip between two perſons according to any table of obſervations. If one of the lives be not very young ſo that the equidiſtant ordinates may not be too few, this area is ſufficiently correct. But if the eldeſt of the two lives is under 20 years of age, it becomes neceſſary to aſſume ſo many equidiſtant ordinates to render the ſolution accurate, when the decrements of life are unequal, that the operation is rendered much too laborious for uſe; nor do I know that it can be neceſſary to have recourſe to this area in any caſe, eſpecially as the true probabilities of ſurvivorſhip are ſo eaſily computed from the preceding ſeries.

The following table has been formed in the manner deſcribed above; and as no ſuch table has ever been attempted before, I have been the more deſirous to render it complete, by computing the probabilities of ſurvivorſhip between two perſons of all ages, whoſe common difference is not leſs than ten years.

Inſtead alſo of ſuppoſing certainty to be denoted by unity, I have aſſumed 100 for this purpoſe; ſo that the ſums in the adjoining columns expreſs the number of chances in 100 which are in favour of B or A's ſurviving the other.

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* Lemma 2. p. 100.

TABLE, ſhewing the probabilities of ſurvivorſhip between two perſons of all ages, whoſe common difference of age is not leſs than ten years, computed from the Northampton Table of Obſervations in Dr. PRICE's Treatiſe on Reverſionary Payments.

Ten years difference.			Twenty years difference.			Thirty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
11 — 1	58.59	41.41	21 — 1	52.46	47.54	31 — 1	48.23	51.77
12 — 2	51.36	48.64	22 — 2	44.33	55.67	32 — 2	39.36	60.64
13 — 3	48.23	51.77	23 — 3	40.88	59.12	33 — 3	35.57	64.43
14 — 4	45.98	54.02	24 — 4	39.00	61.00	34 — 4	32.86	67.14
15 — 5	44.70	55.30	25 — 5	37.69	62.31	35 — 5	31.35	68.65
16 — 6	43.43	56.57	26 — 6	36.40	63.60	36 — 6	29.85	70.15
17 — 7	42.53	57.47	27 — 7	35.48	64.52	37 — 7	28.83	71.17
18 — 8	41.91	58.09	28 — 8	34.84	65.16	38 — 8	27.98	72.02
19 — 9	41.60	58.40	29 — 9	34.52	65.48	39 — 9	27.53	72.47
20 — 10	41.53	58.47	30 — 10	34.41	65.59	40 — 10	27.33	72.67
21 — 11	41.58	58.42	31 — 11	34.40	65.60	41 — 11	27.24	72.76
22 — 12	41.68	58.32	32 — 12	34.42	65.58	42 — 12	27.18	72.82
23 — 13	41.78	58.22	33 — 13	34.44	65.56	43 — 13	27.14	72.86
24 — 14	41.90	58.10	34 — 14	34.47	65.53	44 — 14	27.11	72.89
25 — 15	42.02	57.98	35 — 15	34.51	65.49	45 — 15	27.08	72.92
26 — 16	42.16	57.84	36 — 16	34.56	65.44	46 — 16	27.06	72.94
27 — 17	42.26	57.74	37 — 17	34.58	65.42	47 — 17	27.01	72.99
28 — 18	42.32	57.68	38 — 18	34.54	65.46	48 — 18	26.90	73.10
29 — 19	42.34	57.66	39 — 19	34.44	65.56	49 — 19	26.72	73.28
30 — 20	42.31	57.69	40 — 20	34.29	65.71	50 — 20	26.50	73.50
31 — 21	42.22	57.78	41 — 21	34.08	65.92	51 — 21	26.21	73.79
32 — 22	42.09	57.91	42 — 22	33.84	66.16	52 — 22	25.89	74.11
33 — 23	41.97	58.03	43 — 23	33.59	66.41	53 — 23	25.56	74.44
34 — 24	41.83	58.17	44 — 24	33.35	66.65	54 — 24	25.22	74.78
35 — 25	41.70	58.30	45 — 25	33.09	66.91	55 — 25	24.87	75.13
36 — 26	41.56	58.44	46 — 26	32.83	67.17	56 — 26	24.52	75.48
37 — 27	41.41	58.59	47 — 27	32.56	67.44	57 — 27	24.16	75.84
38 — 28	41.26	58.74	48 — 28	32.28	67.72	58 — 28	23.79	76.21
39 — 29	41.10	58.90	49 — 29	31.99	68.01	59 — 29	23.41	76.59
40 — 30	40.94	59.06	50 — 30	31.70	68.30	60 — 30	23.02	76.98
41 — 31	40.78	59.22	51 — 31	31.43	68.57	61 — 31	22.63	77.37
42 — 32	40.63	59.37	52 — 32	31.16	68.84	62 — 32	22.23	77.77
43 — 33	40.49	59.51	53 — 33	30.88	69.12	63 — 33	21.81	78.19

Ten years difference.			Twenty years difference.			Thirty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
44—34	40.34	59.66	54—34	30.60	69.40	64—34	21.38	78.62
45—35	40.19	59.81	55—35	30.32	69.68	65—35	20.93	79.07
46—36	40.04	59.96	56—36	30.03	69.97	66—36	20.47	79.53
47—37	39.88	60.12	57—37	29.73	70.27	67—37	20.01	79.99
48—38	39.71	60.29	58—38	29.43	70.57	68—38	19.54	80.46
49—39	39.54	60.46	59—39	29.13	70.87	69—39	19.06	80.94
50—40	39.38	60.62	60—40	28.82	71.18	70—40	18.59	81.41
51—41	39.23	60.77	61—41	28.49	71.51	71—41	18.10	81.90
52—42	39.07	60.93	62—42	28.14	71.86	72—42	17.58	82.42
53—43	38.90	61.10	63—43	27.74	72.26	73—43	17.05	82.95
54—44	38.81	61.19	64—44	27.33	72.67	74—44	16.53	83.47
55—45	38.53	61.47	65—45	26.90	73.10	75—45	16.04	83.96
56—46	38.35	61.65	66—46	26.46	73.54	76—46	15.60	84.40
57—47	38.16	61.84	67—47	26.01	73.99	77—47	15.15	84.85
58—48	37.97	62.03	68—48	25.55	74.45	78—48	14.68	85.32
59—49	37.77	62.23	69—49	25.09	74 91	79—49	14.16	85.84
60—50	37.56	62.44	70—50	24.61	75.39	80—50	13.61	86.39
61—51	37.30	62.70	71—51	24.06	75.94	81—51	13.04	86.96
62—52	37.00	63.00	72—52	23.49	76.51	82—52	12.46	87.54
63—53	36.68	63.32	73—53	22.91	77.09	83—53	11.94	88.06
64—54	36.34	63.66	74 - 54	22.35	77.65	84—54	11.60	88.40
65—55	35.97	64.03	75—55	21.81	78.19	85—55	11.30	88.70
66—56	35.58	64.42	76—56	21.31	78.69	86—56	11.04	88.96
67—57	35.17	64.83	77—57	20.80	79.20	87—57	10.80	89.20
68—58	34.74	65.26	78—58	20.24	79.76	88—58	10.63	89.37
69—59	34.30	65.70	79—59	19.59	80.41	89—59	10.25	89.75
70—60	33.85	66.15	80—60	18.90	81.10	90—60	9.65	90.35
71—61	33.38	66.62	81—61	18.23	81.77	91—61	8.68	91.32
72—62	32.90	67.10	82—62	17.57	82 43	92—62	7.54	92.46
73—63	32.46	67.54	83—63	17.03	82.97	93—63	6.18	93.82
74—64	32.04	67.96	84—64	16.73	83.27	94—64	4.93	95.07
75—65	31.70	68 30	85—65	16.55	83.45	95—65	3.68	96.32
76—66	31.45	68.55	86—66	16.29	83.71	96—66	2.58	97.42
77—67	31.20	68.80	87—67	16.38	83.62			
78—68	30.90	69.10	88—68	16.44	83.56			
79—69	30.47	69.53	89—69	16,23	83.77			
80—70	30.00	70.00	90—70	15.67	84.33			
81—71	29.58	70.42	91—71	14.50	85.50			
82—72	29.19	70 81	92—72	13.05	86.95			
83—73	28 90	71.10	93—73	11 08	88.92			
84—74	29 02	70 98	94—74	9.24	90.76			
85—75	29.17	70.83	95—75	7.17	92.83			
86—76	29.25	70.75	96—76	5.12	94.88			

Ten years difference.

Ages.	Probabilities.	
87—77	29.42	70.58
88—78	29.94	70.06
89—79	30.29	69.71
90—80	30.22	69.78
91—81	28.97	71.03
92—82	27.20	72.80
93—83	24.20	75.80
94—84	20.49	79.51
95—85	16.06	83.94
96—86	11.73	88.27

Forty years difference.			Fifty years difference.			Sixty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
41— 1	43.57	56.43	51— 1	39.16	60.84	61— 1	34.94	65.06
42— 2	33.98	66.02	52— 2	28.87	71.13	62— 2	24.13	75.87
43— 3	29.85	70.15	53— 3	24.46	75.54	63— 3	19.52	80.48
44— 4	26.88	73.12	54— 4	21.28	78.72	64— 4	16.19	83.81
45— 5	25.20	74.80	55— 5	19.48	80.52	65— 5	14.28	85.72
46— 6	23.53	76.47	56— 6	17.68	82.32	66— 6	12.37	87.63
47— 7	22.30	77.70	57— 7	16.35	83.65	67— 7	10.92	89.08
48— 8	21.40	78.60	58— 8	15.35	84.65	68— 8	9.82	90.18
49— 9	20.86	79.14	59— 9	14.74	85.26	69— 9	9.12	90.88
50—10	20.59	79.41	60—10	14.42	85.58	70—10	8.73	91.27
51—11	20.45	79.55	61—11	14.22	85.78	71—11	8.46	91.54
52—12	20.35	79.65	62—12	14.06	85.94	72—12	8.25	91.75
53—13	20.26	79.74	63—13	13.90	86.10	73—13	8.05	91.95
54—14	20.18	79.82	64—14	13.76	86.24	74—14	7.88	92.12
55—15	20.11	79.89	65—15	13.62	86.38	75—15	7.75	92.25
56—16	20.05	79.95	66—16	13.50	86.50	76—16	7.68	92.32
57—17	19.96	80.04	67—17	13.35	86.65	77—17	7.59	92.41
58—18	19.81	80.19	68—18	13.14	86.86	78—18	7.43	92.57
59—19	19.59	80.41	69—19	12.86	87.14	79—19	7.19	92.81
60—20	19.31	80.69	70—20	12.53	87.47	80—20	6.90	93.10
61—21	18.96	81.04	71—21	12.11	87.89	81—21	6.55	93.45
62—22	18.55	81.45	72—22	11.65	88.35	82—22	6.16	93.84
63—23	18.12	81.88	73—23	11.20	88.80	83—23	5.81	94.19
64—24	17.68	82.32	74—24	10.75	89.25	84—24	5.56	94.44

Forty years difference.			Fifty years difference.			Sixty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
65–25	17.22	82.78	75–25	10.32	89.68	85–25	5.32	94 68
66–26	16.76	83.24	76–26	9.91	90.09	86–26	5.11	94.89
67–27	16.29	83.71	77–27	9.50	90.50	87–27	4.90	95.10
68–28	15.81	84.19	78–28	9.07	90.93	88–28	4.73	95.27
69–29	15.32	84.68	79–29	8.61	91.39	89–29	4.48	95.52
70–30	14.83	85.17	80–30	8.15	91.85	90–30	4.13	95 87
71–31	14.34	85.66	81–31	7.70	92.30	91–31	3.63	96.37
72–32	13.84	86 16	82–32	7.27	92.73	92–32	3.10	96.90
73–33	13.35	86.65	83–33	6.88	93.12	93–33	2.48	97.52
74–34	12.87	87.13	84–34	6.60	93 40	94–34	1.94	98.06
75–35	12.42	87.58	85–35	6.35	93.65	95–35	1.40	98.60
76–36	11.99	88.01	86–36	6.12	93.88	96–36	0.95	99.05
77–37	11.56	88 44	87–37	5.91	94.09			
78–38	11.12	88.88	88–38	5.74	94.26			
79–39	10.64	89.36	89–39	5.48	94.52			
80–40	10.21	89.79	90–40	5.11	94.89			
81–41	9.68	90.32	91–41	4.55	95.45			
82–42	9.19	90.81	92–42	3.92	96.08			
83–43	8.76	91.24	93–43	3.15	96.85			
84–44	8.46	91.54	94–44	2.47	97.53			
85–45	8.18	91.82	95–45	1.80	98.20			
86–46	7.95	92.05	96–46	1.23	98.77			
87–47	7.74	92.26						
88–48	7.59	92.41						
89–49	7.32	92.68						
90–50	6.90	93.10						
91–51	6.17	93.83						
92–52	5.33	94.67						
93–53	4.32	95.68						
94–54	3.42	96.58						
95–55	2.51	97.49						
96–56	1.73	98.27						

Seventy years difference.			Eighty years difference.			Ninety years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
71— 1	30.49	69.51	81— 1	24.95	75.05	91— 1	18.93	81.07
72— 2	19.45	80.55	82— 2	14.37	85.63	92— 2	9.18	90.82
73— 3	14.87	85.13	83— 3	10.34	89.66	93— 3	5.53	94.46
74— 4	11.59	88.41	84— 4	7.63	92.37	94— 4	3.12	96.88
75— 5	9.80	90.20	85— 5	6.34	93.66	95— 5	2.12	97.88
76— 6	7.98	92.02	86— 6	4.92	95.08	96— 6	1.15	98.85
77— 7	6.60	93.40	87— 7	3.85	96.15			
78— 8	5.53	94.47	88— 8	3.04	96.96			
79— 9	4.84	95.16	89— 9	2.50	97.50			
80—10	4.44	95.56	90—10	2.16	97.84			
81—11	4.18	95.82	91—11	1.86	98.14			
82—12	3.97	96.03	92—12	1.57	98.43			
83—13	3.80	96.20	93—13	1.25	98.75			
84—14	3.72	96.28	94—14	0.97	99.03			
85—15	3.68	96.32	95—15	0.70	99.30			
86—16	3.70	96.30	96—16	0.49	99.51			
87—17	3.73	96.27						
88—18	3.75	96.25						
89—19	3.67	96.33						
90—20	3.45	96.55						
91—21	3.09	96.91						
92—22	2.63	97.37						
93—23	2.10	97.90						
94—24	1.64	98.36						
95—25	1.18	98.82						
96—26	0.80	99.20						

PROBLEM II.

Suppoſing the ages of A and B to be given; to determine, from any table of obſervations, the preſent value of the ſum S payable on the contingency of one life's ſurviving the other.

SOLUTION.

Let r denote £ 1. increaſed by its intereſt for a year, and let all the other ſymbols be the ſame as in the preceding problem. Let the life of B alſo be ſuppoſed to be the older of the two lives; and then it will follow, by reaſoning as in the ſolution of that problem, that the preſent value of S to be received on death of A, ſhould that happen in the life-time of B, will be expreſſed by the ſeries $S \times \overline{\frac{\overline{b+c}\,.\,a'}{2abr} + \frac{\overline{c+d}\,.\,a''}{2abr^2} + \frac{\overline{d+e}\,.\,a'''}{2abr^3} + \frac{\overline{c+f}\,.\,a''''}{2abr^4}}$ &c. This ſeries may be reſolved into the two following; $\frac{S}{2} \times \overline{\frac{ca'}{abr} + \frac{da''}{abr^2} + \frac{ea'''}{abr^3} + \frac{fa''''}{abr^4}}$ &c. $+ \frac{S}{2} + \overline{\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^2} + \frac{fa''''}{abr^4}}$ &c. The firſt of theſe two ſeries may be again reſolved into $\frac{S}{2} \times \overline{\frac{c}{br} - \frac{\overline{ca-ca'}}{abr} + \frac{d}{br^2} - \frac{\overline{da-da'-da''}}{abr^2} + \frac{e}{br^3} - \frac{\overline{ea-ea'-ea''-ea'''}}{abr^3}}$ &c. $\left(-\frac{S}{2} \times \overline{\frac{da'}{abr^2} + \frac{ea'+ea''}{abr^3}}\right.$ &c. $\left.=\right) - S \times \frac{c}{2br} \times \overline{\frac{d}{cr} - \frac{\overline{da-da'}}{acr} + \frac{e}{cr^2} - \frac{\overline{ea-ea'-ea''}}{acr^2}}$ &c. Let B denote the value of an annuity on the life of B, C the value of an annuity on a life one year older than B, AB and AC the values of annuities on the joint lives of A and B and of A and C, and theſe ſeries will be $= \frac{S \times \overline{B-AB}}{2} - \frac{S \times c \times \overline{C-AC}}{2br}$. Again, the ſecond ſeries above mentioned, or $\frac{S}{2} \times \overline{\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3}}$ &c., by purſuing the ſame ſteps may be found $= \frac{\beta \times S}{2b} \times \overline{K-AK} - \frac{S\,.\,\overline{B-AB}}{2r}$ where β denotes the number of perſons living at the age of a perſon one year younger than B, K the value of an annuity on that life, and AK the value of an annuity on the joint lives of A and K. The whole value of the ſurvivorſhip is therefore =

$$S \times \overline{\frac{\overline{r-1} \cdot \overline{B-AB}}{2r} + \frac{\beta \cdot \overline{K-AK}}{2b} - \frac{c \cdot \overline{A-AC}}{2br}}.$$ Q. E. D.

Having now the value of the ſum S payable on the contingency of B's ſurviving A, the value of the ſame ſum, payable on the contingency of A's ſurviving B, is eaſily obtained by the well known method of ſubtracting the value found above from the whole value of the reverſion after the extinction of the joint lives of A and B. It is evident, that the exactneſs of the above rule muſt depend on the accuracy with which the values of the ſingle and joint lives are computed. Being poſſeſſed of ſuch tables for all ages, even with reſpect to the joint lives, I have computed the following values, in order that it may be ſeen how far Mr. SIMPSON's approximation * (the only rule now in uſe) may be depended on.

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* It muſt be remembered, that the correction explained by Dr. PRICE, in Vol. I. p. 39, &c. of his Treatiſe on Reverſionary Payments, muſt be applied to Mr. SIMPSON's rule; that is, when the reverſion is a *ſum* and not an eſtate, the value found by the rule muſt be divided by £. 1, increaſed by its intereſt for a year.

Age of B.	Age of A.	* Value of £. 100 payable on the death of A if B ſurvives him.		Age of B.	Age of A.	Value of £. 100 payable on the death of A if B ſurvives him.		Age of B.	Age of A.	Value of £. 100 payable on the death of A if B ſurvives him.	
		True value.	SIMPSON's value.			True value.	SIMPSON's value.			True value.	SIMPSON's value.
10	2	32.67	26.05	40	10	17.10	19.07	60	60	38.88	38.92
10	10	24.74	24.75	40	40	32.86	32.87	70	2	18.36	9.81
20	2	29.99	24 73	50	2	23.36	17.06	70	10	7.07	9.15
20	10	22.11	23.50	50	10	14.01	16.21	70	40	15.35	15.78
20	20	27.95	27.96	50	20	18.65	19.29	70	70	42.34	42.33
30	2	28.79	22.60	50	50	35.80	35.85	80	2	14.46	5.71
30	10	19.84	21.47	60	2	21.52	13.61	80	10	3.93	5.43
30	30	30.22	30.21	60	10	10.65	12.93	80	50	12.05	12.00
40	2	26.65	20.07	60	30	17.51	18.19	80	80	45.47	45.45

From this table it appears, that Mr. SIMPSON's approximation in the middle ſtages of life is ſufficiently accurate; but that it is exceedingly defective when the life of A is very young. It ſhould alſo be remembered, that theſe values have been computed at a low rate of intereſt, and from the *Northampton* Table of Obſervations, in which the decrements of life come nearer to M. DE MOIVRE's hypotheſis than in any other table. But if the computations be made at a higher rate of intereſt even from this table, the approximation does not always agree ſo well, as will appear from the following ſpecimens calculated at 5 *per cent.*

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* Theſe values have been computed at 3 *per cent.* and from the Northampton Table of Obſervations.

Age of B	Age of A	Value of £. 100 payable on the death of A if B ſurvives him, by the Northampton table.		Value of £. 100 payable on the death of A if B ſurvives him, according to the Sweden Table of Obſervations, and at 4 *per cent.*							
		True value.	Simpson's value.	Age of B.	Age of A.	True value	Simpson's value.	Age of B.	Age of A.	True value.	Simpson's value.
20	2	25.09	18.46	20	2	21.47	16.80	60	36	12.29	16.81
20	10	15.49	17.54	20	14	15.42	17.82	60	42	16.11	19.58
40	2	23.57	15.97	20	20	20.01	19.84	60	60	36.96	36.34
60	2	19.83	11.75	40	4	23.53	14.22	76	40	9.21	9.81
60	40	18.73	19.61*	40	16	13.71	16.23	76	52	12.58	14.00
				40	28	17.60	20.44	76	64	23.81	22.81
				40	40	27.62	27.00	76	76	42.90	43.29
				60	24	9.39	13.01				

In order further to compare Mr. Simpson's approximation with the true value, I have inſerted in the foregoing table a few computations deduced from the *Sweden* Table of Obſervations, in which the decrements of life are unequal. From theſe inſtances the approximation appears to be more defective in proportion as the probabilities of life differ from the hypotheſis. It may be proper, however, to obſerve, that this table has been computed from the values of two joint lives given by Dr. Price in his Treatiſe on Reverſionary Payments, which values cannot be found with perfect correctneſs at every age, becauſe given in that Treatiſe (Vol. II. p. 57, &c. and p. 144, &c.) only for ages that are either equal, or whoſe difference is five years or ſome multiple of five as in the *Northampton* Table, or ſix years or ſome multiple of ſix as in the *Sweden* Table. This circumſtance may perhaps be ſuppoſed to affect the values of the ſurvivorſhips computed from them, when the joint lives differ from each other in a greater or leſs degree than they have been given in thoſe Tables. But by comparing the values of the reverſions when the ages are equal (in which caſe Mr. SIMPSON gives the true rule) it will be ſeen, that the values of the joint lives are ſufficiently correct for the purpoſe; and in order to put this matter out of all doubt, I have computed the values by the foregoing rule from the *exact* values of the joint lives by the *Northampton* Table, and alſo from thoſe values which have been deduced from Dr. PRICE's Tables, and I have found them to agree exceedingly well*.

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* Theſe values at 3 *per cent.* are 21.92 and 22.78.

It may be perceived, that in all the caſes of *equal* lives I have computed the values of the ſurvivorſhips by the preceding rule. This has only been done to prove its accuracy by comparing it with Mr. SIMPSON's rule, which is in this particular caſe, as I have already obſerved, perfectly right; for when the ages are equal, the chance of ſurvivorſhip muſt alſo be equal, and therefore *half* the value of the reverſion after the extinction of the joint lives will be the true value of the given ſum payable on the death of either A or B, ſubject to the contingency of his being ſurvived by the other life. The truth of this rule is ſelf-evident; nor does it at all ſeem to depend on Mr. SIMPSON's ſolution in his Select Exerciſes. That it is capable, however, of being deduced from the foregoing ſeries, may be demonſtrated in the following manner. By reaſoning as in p. 335. the value of the ſum S, when the ages are equal, will be $= S \times \overline{\frac{c}{br} \times \frac{b-c}{b} + \frac{\overline{b-c}|^2}{2bbr}} + \overline{\frac{d}{br^2} \times \frac{c-d}{b} + \frac{\overline{c-d}|^2}{2bbr^2}} + \&c. =$ $\frac{S}{2} \times \overline{\frac{bb}{bbr} + \frac{cc}{bbr^2} + \frac{dd}{bbr^3} + \&c.} - \frac{S}{2} \times \overline{\frac{cc}{bbr} + \frac{dd}{bbr^2} + \frac{ae}{bbr^3} + \&c.} = \frac{S}{2r} + \overline{\frac{S}{2r} - \frac{S}{2}}$ $\times$ BB (putting BB for the value of the two equal joint lives) $=$ $\frac{S}{2} \times \overline{\frac{1}{r} - \frac{\overline{r-1}}{r} \times BB} =$ (ſince the perpetuity P is $\frac{1}{r-1}$, and the intereſt of any ſum is that ſum multiplied by $r-1$) $\frac{S}{2} \times \overline{\frac{P.\overline{\ \ -1}}{r} - \frac{BB.\overline{\ \ -1}}{r}}$, which is Mr. SIMPSON's rule with Dr. PRICE's correction; that is, "half the ſum multiplied by the "difference between the perpetuity and the value of the equal "joint lives, and divided by £. 1 increaſed by its intereſt for a "year."

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* I ſhall juſt mention the following inſtances. When the age of B is 20 and that of A is 2 years, the value, by taking the joint lives from Dr. PRICE's Table, and approximating to the real values of thoſe lives in the manner directed by him, Vol. II. p. 75. of his Treatiſe, is 29.59. When the ages of B and A are 40 and 2, 50 and 20, 60 and 2, and 70 and 10, the reſpective values are 26.93, 18.78, 21.57, and 7.08, which being compared with the values in p. 344. will be found to agree almoſt exactly with them.

PROBLEM III.

The ages of A and B being given; to determine the value of the ſum S, payable on the extinction of one life in particular, ſhould that happen after the extinction of the other life.

SOLUTION.

Suppoſing B to be the oldeſt of the two lives and the ſum S to become payable on his deceaſe, it is evident that this payment at the end of the firſt year muſt depend on the contingency of both lives being extinct before this period and of B's dying *laſt*. Retaining the ſame ſymbols, and reaſoning as I have done in the ſolution of the firſt problem, this value

will

will be expreſſed by the fraction $\frac{S\,.\,a'\,.\,\overline{b-c}}{2abr}$. The payment of the ſum S at the end of the ſecond year will depend on either of two events happening. Firſt, that A and B both die in the ſecond year after having ſurvived the firſt, reſtrained, as above, to the contingency of B's having died laſt; ſecondly, that B dies in the ſecond year and A in the firſt year. The value therefore of S for this year will be expreſſed by the two fractions $\frac{S\,.\,a''\,.\,\overline{c-d}}{2abr^2}+\frac{S\,.\,\overline{c-d}\,.\,a'}{abr^2}$. Again, the payment of S in the third year will depend either on A and B's both dying in that year, and B having died *laſt*; or on B's dying in that year, and A's dying in the firſt or ſecond years. The value therefore of S for this year will be $=\frac{S\,.\,a'''\,.\,\overline{d-e}}{2abr^3}+\frac{S\,.\,\overline{d-e}\,.\,\overline{a'+a''}}{abr^3}$. By proceeding in this manner for the other years the whole value of the reverſion will be found $=\frac{S}{2}\times\overline{\frac{a'\,.\,\overline{b-c}}{abr}+\frac{a''\,.\,\overline{c-d}}{abr^2}+}$ $\overline{\frac{a'''\,.\,\overline{d-e}}{abr^3}+\frac{a''''\,.\,\overline{e-f}}{abr^4}}+\&c.+S\times\overline{\frac{a'\,.\,\overline{c-d}}{abr^2}+\frac{\overline{a'+a''}\,.\,\overline{d-e}}{abr^3}+\frac{\overline{a'+a''+a'''}\,.\,\overline{e-f}}{abr^4}}$ $\overline{+\&c.}$ The firſt of theſe ſeries by proceeding in the ſame manner as in the ſolution of the ſecond problem may be found $=$ $\frac{\beta}{2b}\times\overline{K-AK}-\frac{\overline{B-AB}}{2r}-\frac{1}{2}\,.\,\overline{B-AB}+\frac{c}{2br}\times\overline{C-AC}$; and the ſecond ſeries may be found $=-\frac{c}{br}\times\overline{C-AC}+\frac{\overline{B-AB}}{r}$. Hence the whole value of the reverſion will be $=S\times\overline{\frac{\beta r\,.\,\overline{K-AK}-c\,.\,\overline{C-AC}}{2br}}$ $\overline{-\frac{\overline{r-1}\,.\,\overline{B-AB}}{2r}}$. Q. E. D.

Having now the value of the ſum S depending on the older of the two lives dying laſt, the value of the ſame ſum depending on the younger of the two lives dying laſt is eaſily obtained, by ſubtracting the value firſt found from the whole value of the reverſion after the extinction of both lives.

The anſwers computed by this rule differ rather more from thoſe computed by Mr. Simpson's approximation than they do in the preceding problem. But I am fearful of becoming tedious, and therefore ſhall deſiſt from inſerting the comparative values in this caſe. It may not, however, be improper to exemplify the truth of this demonſtration, by ſhewing in what manner the ſeries may be reſolved into the plain ſimple rule for computing the value of the reverſion, when the ages are equal. The ſeries in this caſe become $\frac{S\cdot\overline{b-c}\rceil^2}{2bbr}+\frac{S\cdot\overline{c-d}\rceil^2}{2bbr^2}$, &c. $+\frac{S\cdot\overline{b-c}\cdot\overline{c-d}}{bbr^2}+\frac{S\cdot\overline{b-d}\cdot\overline{d-e}}{bbr^3}+$ &c. $=$ $\frac{S}{2}\times\overline{\frac{bb}{bbr}+\frac{cc-2bc}{bbr}+\frac{dd-2bd}{bbr^2}+\text{\&c.}}+\frac{S}{2}\times\overline{\frac{2bc-cc}{bbr^2}+\frac{2bd-dd}{bbr^3}}$, &c. $=$ $S\times\overline{\frac{1}{2r}+\frac{1}{r}\times\frac{\overline{2B-BB}}{2}-\frac{\overline{2B-BB}}{2}}=$ (putting L for $2B-BB$, the value of an annuity on the longeſt of the two equal lives) $\frac{S}{2}\times\overline{\frac{1}{r}+\frac{\overline{1-r}\cdot L}{r}}=\frac{S}{2}\times\overline{\frac{P\cdot\overline{r-1}}{r}-\frac{L\cdot\overline{r-1}}{r}}$. Q. E. D*.

The *exact* value of all reverſions depending on ſurvivorſhips between two lives might be found in the ſame manner as the values in the preceding problems. With regard to the values of reverſions depending on ſurvivorſhips between three lives, I am ſenſible that the ſolutions of thoſe caſes would be rather difficult when deduced from the real probabilities of life. But they certainly might be effected; and *theſe* are the more neceſſary, inaſmuch as the ſolutions derived from the expectations of life are often ſo very defective as not to deſerve the name of approximations.

Chatham Place, Feb. 2, 1788.

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* See the latter part of the ſolution of the ſecond problem, p. 347.

XXI. *An Account of a remarkable Tranſpoſition of the Viſcera. By* Matthew Baillie, *M. D. In a Letter to* John Hunter, *Eſq. F. R. S.*

Read May 8, 1788.

TO JOHN HUNTER, ESQ. F.R S.

DEAR SIR,

A VERY ſingular variety having occurred lately in the ſtructure of the human body, I beg leave to communicate an account of it by your means to the Royal Society, if you ſhould think it worthy of their notice. It happens by a very uncommon concurrence of circumſtances, that while I am naturally led by the ties of affinity to apply to you upon this occaſion, I can gratify my pride by thinking it is at the ſame time to the perſon who is poſſeſſed of the firſt reputation for his unwearied reſearches in one of the moſt extenſive, as well as intereſting, parts of the ſyſtem of nature.

I am, &c.

M. BAILLIE.

Great Windmill-ſtreet,
April 12, 1788.

THERE is nothing which tends more to illuſtrate the powers and the wiſdom of nature than the inveſtigation of the ſtructure of animals. We there find a moſt wonderful delicacy of mechaniſm, and exquiſitely adapted to a variety of purpoſes. This, however, is not to be better ſeen by following nature in her common tract than by obſerving her wanderings. In theſe ſhe often ſhews more particularly the extent of her powers, and throws light on her ordinary plans. It is ſuch circumſtances which give importance or value to the obſervation of ſingular phænomena. The variety in animal ſtructure, an account of which I have the honour of preſenting to this learned Society, is a complete tranſpoſition in the human ſubject of the thoracic and abdominal viſcera to the oppoſite ſide from what is natural.

It is ſo extraordinay as ſcarcely to have been ſeen by any of the moſt celebrated anatomiſts, and indeed has been but very generally noticed at all. The circumſtance has been mentioned, but it has not been particularly deſcribed ſo as to make it thoroughly known, or to eſtabliſh its certainty. It was hanging in the minds of many as doubtful, whether ſuch a variety did really exiſt. There is one circumſtance that attends the account of the preſent caſe, which has not always happened in the record of ſingular phænomena, *viz.* that it has been examined by phyſicians and ſurgeons of the firſt reputation in this large town, and has been in ſome meaſure open to the gratification of public curioſity. Such a circumſtance (were it neceſſary) would give the ſtamp of authenticity to appearances even much farther removed from the ordinary plans of nature.

I have been at the pains to conſult many authors upon this ſubject, but with very little ſatisfaction. I ſhall not enter into a detail of what I have met with in the courſe of theſe reſearches, but ſhall briefly notice, that when any luſus of this ſort is mentioned, it is commonly in a ſingle ſentence or two, and the tranſpoſition is not marked as univerſal, or it is a change in the ſituation of ſome viſcus from diſeaſe. In ſhort, I have only found this ſingular luſus naturæ deſcribed by CATTIERIUS, M. MERY, and M. DAUBENTON; but by none of them is it ſufficiently particular. Enough has been ſaid to point out that they had exactly met with the ſame ſort of monſtroſity; but many circumſtances have been omitted, which I hope will be ſupplied by the preſent account, which I proceed immediately to lay before the Society.

The perſon who is the ſubject of this Paper was a male, near forty years of age, ſomewhat above the middle ſtature, and of a clean active ſhape. He was brought for diſſection in the common way to Windmill-ſtreet. Upon opening the cavity of the thorax and abdomen, the different ſituation of the viſcera was ſo ſtriking as immediately to excite the attention of the pupils who were engaged in diſſecting it; and Mr. CRUIKSHANK, as well as myſelf, were very ſoon informed of the ſingularity. We were much ſurprized as well as pleaſed with the appearance, and I began immediately to examine every part of the change with conſiderable attention: for this purpoſe, after deſiring a drawing to be made of the appearances as they were found upon opening the body, I next day injected it. The repeated diſſections have furniſhed various views, which are repreſented faithfully by drawings, and which I hope will enable me to give a tolerably diſtinct account of this ſingular luſus naturæ. I ſhall not enter in my deſcription into unneceſſary minutiæ: this would render the

Paper leſs ſuited to the Society, would not convey more information to perſons thoroughly acquainted with anatomy, and would rather tend to obſcure what is more important to thoſe who have not given ſo much attention to ſubjects of this nature. It may not be improper to obſerve, that, beſides the tranſpoſition in the viſcera of this perſon, there are ſeveral peculiarities which ſometimes occur. I have taken notice of them in my deſcription, although they are entirely independent of the tranſpoſition.

Deſcription of the thorax.

The mediaſtinum, or anterior duplicature of the pleura ſeparating the two cavities of the cheſt from each other, was found to incline obliquely downwards to the right ſide fully as much as it does commonly to the left-ſide of the cheſt. The pericardium too inclined obliquely to the right-ſide. On preſſing it gently away from the lungs the phrenic nerves came diſtinctly into view, in their common ſituation; but the right phrenic nerve ran more obliquely, and was longer than the left. The lung upon the right-ſide was divided by a ſingle oblique fiſſure into two lobes, having at the ſame time a deficiency oppoſite to the apex of the heart; and the lung on the left-ſide was divided into three lobes, exactly contrary to what is found in ordinary caſes.

On opening the pericardium the apex of the heart was found to point to the right-ſide nearly oppoſite to the ſixth rib, and its cavities as well as large veſſels were completely tranſpoſed. What are commonly called the right auricle and ventricle were ſituated on the left-ſide, and the left auricle and ventricle on the right. The pulmonary artery aſcended towards the right-ſide of the cheſt. The aorta was alſo directing its arch to the right; and the vena cava ſuperior, as well as inferior, were ſeen opening into their auricle on the left-ſide of the ſpine. There was nothing remarkable in the ſize or general figure of the heart.

On the outſide of the pericardium the tranſpoſition of the larger veſſels was very ſtriking. The longer ſub-clavian vein was paſſing from the left-ſide obliquely to the right before the branches which are ſent off from the arch of the aorta. The left carotid and ſub-clavian arteries were found to ariſe from the arch of the aorta by one common trunk; the right carotid and ſub-clavian ſeparately.

In the duplicature of the pleura behind, or what may be called the poſterior mediaſtinum, there was a change correſponding to what we have already deſcribed. The deſcending aorta was found paſſing on the right ſide of the ſpine. The œſophagus was before it, inclining more and more to the right towards its lower extremity, and it at length perforated the diaphragm ſomewhat on the right-ſide of the ſpine *. The thoracic duct was ſeen in the middle between the deſcending aorta and the vena azygos in ſome places forming a plexus of ſmall branches, in another dividing itſelf into two branches, which afterwards re-united in a common trunk, and at length climbing up to terminate in the angle between the jugular and ſub-clavian veins on the right-ſide of the body. The recurrent nerve of the parvagum on the right-ſide paſſed round the beginning of the deſcending aorta, and on the left paſſed round

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* The vena azygos was on the left-ſide of the ſpine opening in the common way into the vena cava ſuperior, which we formerly mentioned to bo alſo tranſpoſed in its ſituation.

the common trunk of the carotid and ſub-clavian arteries. The large intercoſtal nerves being exactly under the ſame circumſtances on each ſide, it was impoſſible there could be any tranſpoſition in them. It appears then, from the foregoing deſcription, that every thing admitting of ſuch a change was completely tranſpoſed in the thorax.

Of the abdomen.

The liver was ſituated in the left hypochondriac region, the ſmall lobe being towards the right, and the great lobe in the left-ſide. The ligaments uniting it to the diaphragm correſponded to this change, the right tranſverſe ligament being longer, and the left being ſhorter, than uſual. The ſuſpenſory ligament could undergo little change, except being puſhed to the left-ſide along with the liver. On preſſing upwards the liver, ſo as to exhibit its poſterior and under ſurface, the gall bladder was ſeen on the left-ſide preſerving its proper relative ſituation to the great lobe of the liver, and the veſſels of the portæ were found upon diſſection to be tranſpoſed correſponding to the change of circumſtances. The hepatic artery was found climbing up obliquely from the right towards the left, before the lobulus ſpigelii, and entered at the portæ into the ſubſtance of the liver by two or three branches on the right of the other veſſels. The ductus communis cholidochus was on the left of the other veſſels, being formed from the ductus hepaticus and ductus cyſticus in the common way, and it paſſed obliquely downwards on the left, to terminate in the duodenum. What was moſt remarkable, of which indeed I never ſaw or heard of any inſtance before, it terminated in the fore-part of the duodenum. The vena portarum paſſed behind the hepatic artery and ductus communis cholidochus, aſcending obliquely towards the left-ſide.

The ſpleen was ſituated in the right hypochondriac region, adhering to the diaphragm in the common way. What was very remarkable was, there being three ſpleens, nearly of the ſize of a pullet's egg, found adhering to the larger ſpleen by ſhort adheſions, beſides two other ſtill ſmaller ſpleens which were involved in the epiploon at the great end of the ſtomach. I never ſaw ſo many ſmall ſpleens in any one ſubject. The pancreas was found on the right-ſide behind the ſtomach, running obliquely from the ſpleen to the curvature of the duodenum, and had its duct entering in common with the ductus communis cholidochus into the cavity of that inteſtine. The ſplenic veſſels were paſſing along the upper edge of the pancreas to the right-ſide, correſponding to the change of ſituation in the pancreas and ſpleen.

The ſtomach was ſituated on the right-ſide, partly hid by the ſmall lobe of the liver paſſing to the left, and terminating in the pylorus, rather on the left ſide of the ſpine. The duodenum took a moſt ſingular courſe; it firſt paſſed to the right-ſide, behind the ſmall end of the ſtomach; it then turned upon itſelf, towards the left-ſide; it afterwards took its proper ſweep to the right-ſide, paſſing behind the ſuperior meſenteric artery and meſaraica major vein. The meſentery began to be formed on the right-ſide, inſtead of the left, as in ordinary caſes. The ilium terminated in the great inteſtine on the left-ſide, and there was in it a diverticulum of conſiderable ſize, a luſus not unfrequently occurring. The cæcum was ſituated on the left pſoas magnus and iliacus internus muſcles. The tranſverſe arch of the colon paſſed from the left to the right-ſide of the body, and the ſigmoid flexure croſſed over the right pſoas, to get into the cavity of the pelvis.

The kidnies had their veſſels tranſpoſed, as we ſhall remark more particularly afterwards; the renal capſules had undergone no change, as no variety could be produced by a tranſpoſition.

The aorta paſſed between the crura of the diaphragm into the cavity of the abdomen, and adhered in its courſe to the ſpine on the right-ſide of the vena cava inferior. Its branches were directed in their courſe correſponding to the peculiar ſituation of the viſcera. The ſplenic and coronary arteries were paſſing to the right-ſide, and the hepatic artery obliquely to the left. The ſuperior and inferior meſenteric arteries were directed to the right-ſide. There was no change in the ſpermatic arteries, any tranſpoſition in the teſticles (if ſuch a thing could take place) not being capable of affecting them. The lumbar arteries could alſo undergo little change, except that the left lumbar arteries muſt neceſſarily, from the peculiar ſituation of the aorta, be the longeſt. The vena cava inferior perforated the tendinous portion of the diaphragm, and adhered in its courſe to the ſpine on the left-ſide of the aorta.

The right emulgent vein was much longer than uſual, paſſing from the right kidney before the aorta to terminate in the vena cava ſuperior; and the left emulgent much ſhorter, paſſing from the left kidney to the vena cava, which was ſituated on the left ſide of the ſpine. The right ſpermatic vein was found to open into the right emulgent, and the left into the vena cava inferior, about an inch under the left emulgent. The vena portarum was changed from its natural courſe, paſſing obliquely upwards to the left-ſide, and its large branches, *viz.* the vena ſplenica, meſaraica major and minor, were all directed towards the right-ſide of the ſpine.

There was no change in the intercoſtal nerve within the cavity of the abdomen; nor does it ſeem to be capable of being affected by any tranſpoſition of parts. We ſee then, that there was a complete tranſpoſition of the abdominal viſcera, each of them preſerving its proper relative ſituation to the others.

I examined the brain, organs of ſenſe, of generation, the muſcles and blood veſſels of the extremities, but found nothing in them remarkable. Indeed, I had no expectation of it; for all theſe parts are perfectly independent of thoracic or abdominal viſcera; but I did it to ſatisfy myſelf and the curioſity of others, who might wiſh to put ſuch a queſtion, or have ſuch a queſtion ariſing in their minds.

The perſon ſeems to have uſed his right-hand in preference to his left, as is uſually the caſe, which was readily diſcovered by the greater bulk and hardneſs of that hand, as well as the greater fleſhineſs of the arm. It was not to be expected he ſhould be left-handed; but I mention this circumſtance too, with a view to ſatisfy a curioſity which I know has been excited in many who have heard of this luſus.

I have been at conſiderable pains to learn ſomething of the hiſtory of this perſon during life; but the particulars I have heard are applicable only to the circumſtances of common men, having no connection with ſingularity of ſtructure, and therefore, I think, it would be abuſing the time of the Society to give any account of them. One thing it may be right to mention is, that the perſon, while alive, was not conſcious of any uncommon ſituation of his heart; and that his brother, whom I have ſeen, has his heart pointing to the left-ſide as in ordinary caſes. Indeed, there was little reaſon to expect that we ſhould meet with any thing particular in the account of his life. His health could not be affected by ſuch a change of ſituation in his viſcera; nor could there ariſe from it any peculiar ſymptoms of diſeaſe. Still leſs could there be any connection between ſuch a change and his diſpoſitions, or external actions. He might have known that his heart was directed towards the right-ſide; but if we conſider how little every perſon, eſpecially thoſe of the lower claſs, are attentive to circumſtances not very palpable, it was ſcarcely to be expected he ſhould know of it. If I had met with any thing in his life which was at all referable to the ſingularity of ſtructure, I ſhould have been very glad to have gratified the public curioſity by giving an account of it *.

Every ſingular phænomenon in animal ſtructure is worth remarking, even if it ſhould not lead immediately to any uſeful obſervation; but it becomes more important if it ſhould tend to throw any light upon the principles of nature in the formation of animals. It is reaſonable to think, that nature ſhould follow ſome general plan in her operations. There is ſome effect which ſhe has in view, and ſhe will generally employ the ſame means to produce it. In the ſtructure of any animal, her view is to form ſuch a combination of parts as to render the animal fitted for certain purpoſes. She will commonly form the ſame combination where the ſame purpoſes are to be ſerved; or, in other words, there will be the ſame ſtructure in the ſame ſpecies of animals. The ſame effect however may be produced, without a ſtrict adherence to the employment of the ſame means, as we find to be the caſe

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* Since the above luſus has occurred, I have ſeen, in the poſſeſſion of Mr. PAYNE, Surgeon, a fœtus at the full time, with the viſcera tranſpoſed. In the Anatomical Collection of Chriſt Church, in Oxford, there is a heart tranſpoſed that had belonged to a very ſmall fœtus; but the fœtus itſelf is not preſerved.

in all human inventions, and therefore there is no reaſon why nature ſhould not ſometimes deviate from her ordinary plans. Accordingly we find there is much variety in animal ſtructure; but this does not commonly affect the animal functions. Under this reſtriction the variety is ſo great in the appearances of every part of an animal, that it is almoſt impoſſible to examine any two animals of the ſame ſpecies without remarking many differences.

In the bony compages of an animal we find little variety in the extremities of bones where there is the apparatus of a joint, becauſe a particular ſhape is beſt adapted to a particular kind or latitude of motion. In other parts of the bones, where a difference of features is not material, there is great variety, as in the foramina, depreſſions, ridges, and ſutures of bones.

The ſame general rule will apply to variety in muſcles. The principal object is a certain inſertion near a joint, ſo as to give a determined direction of motion. With reſpect to ſuch inſertions, there is, comparatively ſpeaking, little variety; but there is a great difference in the bodies and connections of muſcles, which have no ſhare in the regulation of the motion.

There is no part of an animal where there is a greater latitude of variety than in the diſtribution of blood veſſels. The reaſon of it is very obvious. The only object in the diſtribution of blood veſſels is, to carry blood to every part of the body and bring it back to the heart. The parts of an animal, in order to be ſupported, muſt be viſited by ſucceſſive changes of freſh blood; but it ſurely cannot be an object of importance whether the blood paſſes by one rout or another. Hence the variety in blood veſſels is extremely great. Still, however, there is a method in the deviations of nature, ſo that they may be marked or noted, the ſame varieties occurring in different animals.

It cannot be at all important to the function of a viſcus, whether it be in one maſs, or in ſeparate portions. The ſtructure being the ſame, the ſame action will take place. Hence we often find the two kidnies joined together, forming one maſs; and not unfrequently two or three ſpleens beſides the common one. Neither can it be important, whether a viſcus ſhould always be of the ſame ſhape, becauſe its functions do not depend on ſhape, but on ſtructure: we find accordingly, in this particular, much variety.

There are many of the viſcera which are connected together in their functions, or by the junction of large blood-veſſels, in ſuch a way as to require nearly the ſame relative ſituation among themſelves. This becomes alſo neceſſary in order to preſerve the general ſhape of the animal. Accordingly we find, that when any important viſcus is changed in its ſituation, it affects the ſituation of other viſcera, requiring in them a ſimilar change. We ſaw in the perſon who is the ſubject of this Paper, that a change in the ſituation of the heart and liver was accompanied with a change of ſituation in the ſtomach, ſpleen, pancreas, and in ſhort the whole abdominal viſcera. This, however, is a great deviation in nature; for it is nothing leſs than changing almoſt the whole vital ſyſtem in an animal, and therefore it rarely happens.

In ſuch a change it does not appear, that the functions can be affected, as they depend on ſtructure and ſituation, which are both preſerved. Hence the perſon who is the ſubject of this Paper arrived at the age of maturity, and might have continued to live to an extreme old age.

The human machine might have been conſtructed in this way generally, and under ſuch circumſtances what is now called the natural ſituation of parts would have been as ſingular as the preſent phænomenon.

There appears to be leſs variety in the nervous ſyſtem of animals of the ſame ſpecies than in moſt parts of the body. There is ſcarcely any difference in the appearance of the brain, and much leſs in the diſtribution of the nerves than of the blood-veſſels.

There is alſo little variety in the organs of ſenſe: perhaps the mechaniſm in both theſe is nicer, ſo that a conſiderable deviation would interfere with their peculiar functions.

The moſt common great deviations which nature produces in the ſtructure of an animal are various kinds of monſtroſity, by which the animal becomes often unfit for continuing its exiſtence. Why nature ſhould in its greater deviations fall into a very imperfect formation, much below the ſtandard of her common work, does not appear very obvious.

It ſeems that there might have been many varieties where the functions could have been preſerved. Perhaps it is with a view to check the propagation of great varieties, ſo as to preſerve an uniformity in the ſame ſpecies of animals.

It has been much agitated, whether monſtroſities depend on the original formation, or are produced afterwards in the gradual evolution of an animal.

This does not appear to be a queſtion of much importance; nor perhaps can it be abſolutely determined. But upon the whole it is more reaſonable to think, that the ſame plan of formation is continued from the beginning, than that at any ſubſequent period there is a change in that plan.

It may be obſerved, that it is exactly the ſame creative action which produces the natural ſtructure, or any deviation from it; for in caſes of deviation the action is either carried too far, ceaſes too ſoon, or is diverted into uncommon channels. This will explain the various kinds of monſtroſity from redundancy, deficiency, or tranſpoſition of parts.

XXII. *On the* Georgian *Planet and its Satellites.*

By William Herſchel, *LL.D. F. R. S.*

Read May 22, 1788.

IN a Paper, containing an account of the diſcovery of two ſatellites revolving round the Georgian planet, I have given the periodical times of theſe ſatellites in a general way, and added that their orbits made a conſiderable angle with the ecliptic. It is hardly neceſſary to mention, that it requires a much longer ſeries of obſervations, to ſettle the mean motions of ſecondary planets with accuracy, than I can hitherto have had an opportunity of making; but ſince it will be ſome ſatiſfaction to aſtronomers to be acquainted with ſeveral of the moſt intereſting particulars, as far as they can as yet be aſcertained, I ſhall communicate the reſult of my paſt obſervations; and believe that, conſidering the difficulty of meaſuring objects which require the utmoſt attention even to be at all perceived, the elements here delivered will be found to be full as accurate as we can at this time expect to have them ſettled.

The moſt convenient way of determining the revolution of a ſatellite round its primary planet, which is that of obſerving its eclipſes, cannot now be uſed with the Georgian ſatellites, as will be ſhewn when I come to give the poſition of their orbits; and as to taking their ſituations in many ſucceſſive oppoſitions of the planet, which is likewiſe another very eligible method, that muſt of courſe remain to be done at proper opportunities. The only way then left, was to take the ſituations of theſe ſatellites, in any place where I could aſcertain them with ſome degree of preciſion, and to reduce them afterwards by computation to ſuch other ſituations as were required for my purpoſe.

In January, February, and March, 1787, the poſitions were determined by cauſing the planet to paſs along a wire, and eſtimating the angle a ſatellite made with this wire, by a high magnifying power; but then I could only uſe ſuch of theſe ſituations where the ſatellite happened to be either directly in the parallel of declination, or in the meridian of the planet; or where, at leaſt, it did not deviate above a few degrees from either of them; as it would not have been ſafe to truſt to more diſtant eſtimations. In October I had improved my apparatus ſo far as to meaſure the poſitions by the ſame angular micrometer with which I have formerly determined the relative poſitions of double ſtars *.

In computing the periods of the ſatellites I have contented myſelf with ſynodical appearances, as the poſition of their orbits, at the time when the ſituations were taken from which theſe periods are deduced, was not ſufficiently known to attempt a very accurate ſidereal calculation. By ſix combinations of poſitions at a diſtance of 7, 8, and 9 months of time, it appears that the firſt ſatellite performs a ſynodical revolution round its primary planet in 8 days 17 hours 1 minute and 19,3 ſeconds. The period of the ſecond ſatellite deduced likewiſe from four ſuch combinations, at the ſame diſtance of time, is 13 days 11 hours 5 minutes and 1,5 ſeconds. The

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* For a deſcription of this inſtrument, ſee Phil. Tranſ. Vol. LXXI. p. 500. and Vol. LXXV. p. 46.

combinations of which the above quantities are a mean do not differ much among themſelves; it may therefore be expected that theſe periods will come very near the truth; and, indeed, I have for many months paſt been uſed to calculate the places of the ſatellites by them, and have hitherto always found them in the ſituations where theſe computations gave me reaſon to expect to ſee them.

The epochæ, from which aſtronomers may calculate the poſitions of theſe ſatellites, are October 19, 1787; for the firſt 19 h. 11′ 28″; and for the ſecond 17 h. 22′ 40″. They were at thoſe times 76° 43′ north-following the planet; which, as will be ſhewn in the ſequel, is the place of the greateſt elongation of the ſecond ſatellite; where, conſequently, its real angular ſituation is the ſame as the apparent one. And I have brought the firſt ſatellite to the ſame place, as hitherto there has not been time to diſcriminate the ſituation of its orbit from that of the ſecond.

The next thing to be determined in the elements of theſe ſatellites is their diſtance from the planet; and as we know that, when the periodical times are given, it is ſufficient to have the diſtance of one ſatellite in order to find that of any other, I confined my attention to the diſcovery of the diſtance of the ſecond. As ſoon as I attempted meaſures, it appeared, that the orbit of this ſatellite was ſeemingly elliptical; it became therefore neceſſary, in order to aſcertain its greateſt elongation, to repeat theſe meaſures in all convenient ſituations; the reſult of which was, that on the 18th of March, at 8 h. 2′ 50″, I found the ſatellite at the diſtance of 46″,46; this being the largeſt of all the meaſures I have had an opportunity of taking. Hence, by computation, it appears, that the ſatellite's greateſt viſible elongation from its planet, at the mean diſtance of the Georgium ſidus from the earth, will be 44″,23.

It ought to be mentioned, that in the reduction of this meaſure I have uſed MAYER's tables for the ſun, and the tables publiſhed in the *Connoiſſance des Temps* of the year 1787, reduced to the time of Greenwich, for the Georgian planet.

Very poſſibly this diſtance might not be taken exactly at the time when the ſatellite happened to be at the vertex of the tranſverſe axis of its apparently elliptical orbit; but, from other meaſurements, we have reaſon to conclude, that it could not be far from that point. For inſtance, the 9th of November, at 15 h. 56′ 15″, by a mean of four good meaſures, the ſatellite was 44″,89 from the planet; which, by calculation, reduced to the ſame diſtance of the Georgian ſidus from the earth as the former, gives 41″,33. And likewiſe, the 19th of March, at 7 h. 45′ 59″, the diſtance meaſured 44″,24; which, computed as before, gives 42″,15. Now, we find, when the places are calculated in which the ſatellite happened to be at the times when theſe two meaſures were taken, that they fall on different ſides of the former meaſure, and alſo on oppoſite parts of the ſatellite's orbit; but that nevertheleſs they agree ſufficiently well with the poſition of the tranſverſe axis which we have adopted in the ſequel.

Admitting, therefore, at preſent, that the ſatellite moves in a circular orbit about its planet, we cannot be much out in taking the calculated quantity of 44″,23 for the true meaſure of its diſtance. And, having aſcertained this point, we calculate, by the law of KEPLER, and the aſſigned period of the firſt ſatellite, that its diſtance from the planet muſt be 33″,09. I ought however to remark, that, in this computation, a true ſidereal period ſhould have been uſed; but, as that cannot as yet be had, the trifling inaccuracy thence ariſing may well be excuſed, till, at ſome future opportunity, we may be permitted to repeat theſe calculations in a more rigorous manner.

As we are now upon the ſubject of ſuch parts of the theory of planets as may be determined by calculation, it will not be amiſs to ſee how the quantity of matter and denſity of our new planet will ſtand, when compared with the tables that have been given of the ſame in the other planets; and in order to this, let us admit the following *data* as a foundation for our computation.

The parallax of the ſun 8″,63.

The parallax of the moon 57′ 11″.

Its ſidereal revolution round the earth 27 d. 7 h. 43′ 11″,6.

The mean diſtance of the Georgian planet from the ſun 19,0818.

The mean diſtance of its ſecond ſatellite from the planet 44″,23.

The periodical time of this ſatellite 13 h. 11 d. 5′ 1″,5.

Hence we find, that a ſpectator, removed to the mean diſtance of the Georgian planet from the earth, would ſee the radius of the moon's orbit under an angle of 27″,1866; and if 1, *d*, *t*, repreſent the quantity of matter in the earth, the diſtance of the moon, and its periodical time; M, D, T, be made to ſtand for the ſame things in our new planet and its ſecond ſatellite, we obtain, by known principles, $M = \frac{t^2D^3}{T^2d^3}$. And, conſequently, the quantity of matter in the Georgian planet is to that contained in the earth as 17,740612 to 1.

In order to caculate the denſity, I compare the mean of the four bright meaſures of the planet's diameter 3,″7975 to the

mean

mean of the two dark ones 4″,295; as they are given in my Paper on the diameter and magnitude of the Georgium ſidus, printed in Volume LXXIII. of the Philoſophical Tranſactions, p. 9. 11, 12, 13. Whence we obtain another mean diameter 4″,04625; which is probably the moſt accurate of any that we have hitherto aſcertained. And let us ſuppoſe this meaſure to belong to the ſituations of the earth and of the new planet as they were at 10 o'clock, the 25th of October, 1782; which is about the middle of the ſeveral times when thoſe meaſures from which this is deduced were taken. Then, by the tables already referred to, we compute the diſtance of the two planets from the ſun and the angle of commutation; whence, by trigonometry, we find the diſtance of our new planet from the earth for the ſuppoſed 25th of October; and thence deduce its mean diameter, which is 3″,90554. This, when brought to what it would appear if it were ſeen from the ſun at the earth's mean diſtance, gives 1′ 14″,5246; which, compared with 17″,26, the earth's mean diameter, is as 4,31769 to 1. The Georgium ſidus, therefore, in bulk, is 80,49256 times as large as the earth; and conſequently its denſity leſs than that of the latter in the ratio of ,220401 to 1.

To theſe particulars, though many of them may be of no other uſe than merely to ſatisfy our curioſity, we may alſo add, that the force of gravity, on this planet's ſurface, is ſuch as will cauſe an heavy body to fall through 18,67308 feet in one ſecond of time.

It remains now only, in order to complete our general idea of the Georgian planet, to inveſtigate the ſituation of the orbits of its ſatellites. I have before remarked, that when I came to examine the diſtance of the ſecond, I perceived immediately that its orbit appeared conſiderably elliptical. This induced me to attempt as many meaſures as poſſible, that I might be enabled to come at the proportion of the axes of the apparent ellipſis; and thence argue its ſituation. But here I met with difficulties that were indeed almoſt inſurmountable. The uncommon faintneſs of the ſatellites; the ſmallneſs of the angles to be meaſured with micrometers which required light enough to ſee the wires; the unwieldy ſize of the inſtrument, which, though very manageable, ſtill demanded aſſiſtant hands for its movements, and conſequently took away a great ſhare of my own directing power, a thing ſo neceſſary in delicate obſervations; the high magnifiers I was obliged to uſe by way of rendering the ſpaces and angles to be meaſured more conſpicuous; in ſhort, every circumſtance ſeemed to conſpire to make the caſe a deſperate one. Add to this, that no meaſure could poſſibly ſucceed which had not the moſt beautiful ſky in its favour; and we may eaſily judge how ſcarce the opportunities of taking ſuch meaſures muſt be in the variable climate of this iſland. As far then as a ſmall number of ſelect meaſures will permit, which, out of about twenty-one that were taken, amounts only to five, I ſhall enter into our preſent ſubject of the poſition of the ſecond ſatellite's orbit.

The following table contains in the firſt column the correct mean time when the meaſures were taken. The ſecond gives the quantity of theſe meaſures. In the third column are the ſame meaſures reduced to the mean diſtance of the Georgian planet from the earth. The fourth contains the calculated poſitions of the ſatellite as it would have appeared to be ſituated if it had moved in a circular orbit at rectangles to the viſual ray; and the degrees are numbered from the firſt obſervation ſuppoſed to have been at zero, and are carried round the circle from right to left.

	D.	H.	′	″	″	″	°	′
March	18	8	2	50	46,46	44,23	0	0
	19	7	47	59	44,24	42,15	26	28
	20	7	44	8	40,23	38,37	53	8
April	11	9	18	27	35,32	34,35	283	13
Nov.	9	15	56	15	44,89	42,88	199	59

In the uſe of this table I ſhall partly content myſelf with the conſtruction of a figure, and only apply calculation to the moſt material circumſtances. By the third column we ſee that 44″,23 is the greateſt, and 34″,35 the leaſt, diſtance of the ſatellite. Let therefore an ellipſis be drawn Tab. V. fig. 1. having the tranſverſe and conjugate diameters *cp* and *cv*, in the proportion of the above-mentioned meaſures. About the center *c*, with the radius *cp*, deſcribe the circle PSFN; and ſet off the points March 18, 19, 20. April 11. and Nov. 9. according to the tabular order of degrees, beginning at *p*, the ſuppoſed zero. From theſe points to the tranſverſe draw the ordinates March 19 *s*, 20 *t*, April 11 *x*, Nov. 9 *y*. Then, if the ſatellite moved in a circular orbit at rectangles to the viſual ray, we ſhould have ſeen it at the time given in the table, as the points are placed in the circumference of our circle; but, ſuppoſing the plane of the orbit inclined to the viſual ray, theſe points will be projected in the direction of the ordinates; and, falling on the places *p n m r o*, will form the ellipſis we have delineated. Now, on comparing the tabular meaſures of the third column with the diſtances of *p n m r* and *o* from the center *c*, we find, that they agree full as well as we could expect; and thus, as far as a few obſervations can do, theſe meaſures eſtabliſh the truth of the above hypotheſis.

That we may have a point in our ellipſis from which to depart, I ſhall have recourſe to two meaſures of poſitions. The firſt was taken October 14 d. 16 h. 28′ 42″, when the ſatellite was 66° 3′ ſouth-following the planet. In fig. 2. let *qmv* be a portion of an ellipſis, conſtructed on the ſemi-tranſverſe *cq*, and ſemi-conjugate *cv*, taken as 44,23 to 34,35; *q*MF an arch of a circle, deſcribed with the radius *cq* about the center *c*; *m* the ſituation of the ſatellite in its elliptical orbit, the 14th of October; A its apparent, and M its real place in the circle; F*c* the parallel of the planet. Then we ſhall have, by calculating from the known period, the arch *q*M 45° 17′; and FA, by obſervation, 66° 3′. But from the nature of the ellipſis, as V*c* is to *vc* ſo is M*n* (the tangent of the angle *qc*M to the radius *cn*) to *mn* (the tangent of the angle *qc*A to the ſame radius). Hence *qc*A is found 38° 7′; and therefore A*c*M 7° 10′. That is, when the angle of poſition was taken, the ſatellite appeared to be 7° 10′ leſs advanced in its orbit than it ſhould have done, owing to its motion in an orbit whoſe plane is inclined to the viſual ray. The meaſure therefore corrected, or rather reduced to the circle, inſtead of 66° 3′, will be 58° 53′ ſouth-following; to which, adding the calculated arch *q*A, and from the ſum deducting 90°, we have the poſition *qc*S with the meridian 14° 10′ on the ſouth-preceding ſide. In the ſame manner I proceed with the ſecond meaſure taken October 20 d. 16 h. 7′ 34″; when the ſatellite appeared to be 82° 12′ north-preceding the planet. Here the arch *q*M is 25° 21′, A*c*M 5° 9′; and the meaſure corrected 77° 3′ north-preceding, which gives the inclination of the axis to the meridian 12° 24′ on the north-following ſide. I have no reaſon to prefer either of the meaſures, and therefore take a mean of both, which is 13° 17′ from ſouth-preceding to north-following the meridian, as probably neareſt the truth; and this poſition of the axis we may ſuppoſe to belong to a time which is about the mean of thoſe from which it has been deduced; or October, 17 d. 16 h.

We are, in the next place, to find the angle which the plane of the meridian made at that time with the plane of the orbit of the Georgian planet. To this end we calculate its longitude and latitude for the given time. Then, in fig. 3. where ♋ NE is part of the ſolſtitial colure, *nc* a portion of the orbit, and ♋ *s* of the ecliptic; there is given the arch NE, 23° 28′; E*c*, 89° 27′ 49″,2 the complement of the planet's latitude; and the angle ♋ E*s*, 27° 0′ 52″,2, or planet's diſtance from Cancer. By theſe we find the angle E*c*N between the circle of latitude N*c* and the meridian E*c* 11° 11′ 41″. Now, let *c*, in fig. 4. be the place of the Georgian planet, and G☊*c* a part of its orbit; *e*☊*s* part of the ecliptic; N*c* the meridian; ☊ the place of the planet's aſcending node; *pcq* the poſition of the axis of the apparently elliptical orbit of the ſecond ſatellite; E*c*N the angle of poſition of the Georgium ſidus. Then, by calculation, for the above-mentioned day we have ☊*c*, the planet's diſtance from the node on its orbit 44° 8′ 17″; *c*☊*s* the inclination of its orbit to the ecliptic 46′ 13″; and ☊*sc* a right-angle; whence E*c*☊ 90° 33′ 10″,1 the ſupplement of the angle *sc*☊ is found; from which, taking the angle of poſition N*c*E, before obtained, we have the remaining angle, N*c*G, 79° 21′ 29″,1; or inclination of the planet's orbit to the plane of the meridian, which was required.

From the proportion of the tranſverſe *cp*, fig. 1. to the conjugate *cv*, we calculate the angle *vpc*, which may be either acute or obtuſe. For here I muſt take notice, that obſervations cannot immediately determine whether the ſatellite, in paſſing from *p* through *nmv* to *q*, be in the fartheſt or neareſt part of its

its orbit; as we ſhall preſently ſhew that this orbit is not in a ſituation to permit the ſatellite to ſuffer either eclipſes or occultations for ſome time to come. The angle *vpc*, therefore, if the arch *pvq* be turned towards us will be 129° 2′ 46″,5; but, if directed the contrary way, 50° 57′ 13″,5. There is one circumſtance which will bring on a diſcovery of this particular, without waiting for eclipſes; for if the apparent ellipſis of the ſatellite's orbit ſhould contract in a year or two, we may conclude this arch to lie towards the ſun; if, on the contrary, it opens, we ſhall know that the ſatellite has paſſed through one of its nodes about eight or nine years ago; and that, therefore, we muſt not expect to ſee it eclipſed for more than thirty years to come.

Now, having already determined the poſition of the axis *pc* with reſpect to the meridian, by adding the angles N*cp* and N*c*G, fig. 4. we obtain *pcn*, 92° 38′ 29″,1; and having alſo now calculated the ambiguous angle *npc*, we may reſolve the quadrantal triangle *pcn*, in which the angle *cnp* gives the inclination of the orbit of the ſatellite to the orbit of its planet, which will be 99° 39′ 48″,9, if the ſatellite be approaching to its aſcending node; but 80° 20′ 11″,1, if it be lately paſt the deſcending one.

In the ſame triangle we find the ſide *nc*, which is either 50° 59′ 0″,8 or 129° 0′ 59″,2; and taking theſe quantities, increaſed by ſix ſigns, from the longitude of the planet in its orbit, gives the place of the ſatellite's aſcending node upon the orbit of the planet, either 8 s. 6° 2′ 0″,3, if the preceding arch of the orbit *pmn*, be concave towards the ſun; or 5 s. 18° 0′ 1″,9, if it be convex.

Theſe elements obtained, we reduce them to the ecliptic by reſolving the triangle ☊*nm*, in which we have *m*☊*n*, 46′ 13″;

n☊ the diſtance of the aſcending node of the ſatellite from the deſcending node of the planet 6° 50′ 43″,8 or 84° 52′ 42″,1; and ☊ nm, the inclination of the ſatellite's orbit to that of its planet 99° 39′ 49″,9 anſwering to the former, or 80° 20′ 11″,1 to the latter. In conſequence of this reſolution, we have the place of the aſcending node of the ſatellite upon the ecliptic, $\left\{\begin{matrix}5\text{ s. } 18^\circ & 0' & 3'',9\\ 8\quad\ \ 6 & 2 & 2\ ,3\end{matrix}\right\}$, and its inclination to the ſame $\left\{\begin{matrix}99^\circ & 43' & 53'',3\\ 81 & 6 & 4\ ,4\end{matrix}\right\}$. The orbit being ſituated ſo, that when the planet will be in the aſcending node of this ſatellite, which will happen about the year $\left\{\begin{matrix}1799\\ 1818\end{matrix}\right\}$, the northern half of it will be turned towards the $\left\{\begin{matrix}\text{Eaſt}\\ \text{Weſt}\end{matrix}\right\}$ at the time of its meridian paſſage.

In juſtice to the foregoing calculations I ſhould add, that the reſult of them muſt be conſiderably affected by any ſmall alteration in the meaſures upon which they are founded; the general theory, however, will certainly ſtand good, and a greater perfection in particulars could not have been obtained, unleſs I had waited ſome years, at leaſt, in order to multiply good obſervations. But with objects that are out of the reach of common teleſcopes, and which therefore cannot be much attended to, even by our moſt aſſiduous aſtronomers, a general theory will perhaps nearly anſwer all the ends that may be required of it.

The meaſures of the diſtances were taken by a good parallel-wire micrometer, contrived ſo that one of the wires, which is moveable, can paſs over the other; by which means central meaſures may be obtained with more accuracy than by allowing for the thickneſs of the wires, the aſcertaining of which is liable to ſome difficulties in other conſtructions; but here, as we can note the diviſions on the firſt appearance of light at either ſide of the fixed wire, when the moveable one paſſes over it backwards and forwards, we may very conveniently determine that part of the ſcale to which the zero ought to anſwer in central meaſures. The value of the ſcale was aſcertained by the tranſit of ſtars over the two wires opened to a certain number of diviſions, and a chronometer beating five times in two ſeconds of mean time; and in a number of ſeveral ſets of experiments, the mean of each ſeldom differed ſo much as the 500dth part of a ſecond of ſpace for each diviſion, and theſe are large enough to be ſub-divided and read off, with good exactneſs to tenths; and yet the ſpace anſwering to each part amounts only to 282 milleſimals of a ſecond. The meaſures of the diſtances alſo were as often repeated as the opportunities would permit, and a mean of them has been uſed.

The light of the ſatellites of the Georgian planet is, as we may well expect, on account of their great diſtance, uncommonly faint. The ſecond is the brighteſt of the two, but the difference is not conſiderable; beſides, we muſt allow for the effect of the light of the planet, which is pretty ſtrong within the ſmall diſtances at which they are revolving. I have ſeen ſmall fixed ſtars, as near the planets as the ſatellites, and with no greater light, which, on removal of the planet, ſhone with a conſiderable luſtre, ſuch as I had by no means expected of them. A ſatellite of Jupiter, removed to the diſtance of the Georgian planet, would ſhine with leſs than the 180th part of its preſent light; and may we not conclude, that our new ſatellites would be of a very conſiderable brightneſs if they were brought ſo near as the orbit of Jupiter, and thus appeared

180 times brighter than at preſent? Nay, this is only when we take both the planets at their mean diſtance; for, in their oppoſitions, a ſatellite brought from the ſuperior planet to the orbit of the inferior one, would reflect nearly 250 times the former light; from all which it is evident, that the Georgian ſatellites muſt be of a conſiderable magnitude.

If we draw together the reſults of the foregoing calculations into a ſmall compaſs, they will ſtand as follows:

The firſt ſatellite revolves round the Georgian planet in 8 days 17 hours 1 minute and 19 ſeconds.

Its diſtance is 33″.

And on the 19th of October, 1787, at 19 h. 11′ 28″, its poſition was 76° 43′ north-following the planet.

The ſecond ſatellite revolves round its primary planet in 13 days 11 hours 5 minutes and 1,5 ſeconds.

Its greateſt diſtance is 44″,23.

And on the 19th of October, 1787, its poſition at 17 h. 22′ 40″, was 76° 43′ north-following the planet.

Laſt year its leaſt diſtance was 34″,35; but the orbit is ſo inclined, that this meaſure will change very conſiderably in a few years, and by that alteration we ſhall know which of the double quantities put down for the inclination and node of its orbit are to be uſed.

The orbit of the ſecond ſatellite is inclined to the ecliptic

$\left\{\begin{matrix} 99^\circ\ 43'\ 53{,}3'' \\ 81\quad\ 6\quad\ 4{,}4\ \ \end{matrix}\right\}$.

Its aſcending node is in $\left\{\begin{matrix} \text{18 degrees of Virgo} \\ \text{6 degrees of Sagittarius} \end{matrix}\right\}$.

When the planet paſſes the meridian, being in the node of this ſatellite, the northern part of its orbit will be turned towards the $\left\{\begin{matrix} \text{Eaſt} \\ \text{Weſt} \end{matrix}\right\}$.

The ſituation of the orbit of the firſt ſatellite does not ſeem to differ materially from that of the ſecond.

We ſhall have eclipſes of theſe ſatellites about the year $\left\{\begin{matrix}1799\\1818\end{matrix}\right\}$, when they will appear to aſcend through the ſhadow of the planet almoſt in a perpendicular direction to the ecliptic.

The ſatellites of the Georgian planet are probably not leſs than thoſe of Jupiter.

The diameter of the new planet is 34217 miles.

The ſame diameter ſeen from the earth, at its mean diſtance, is 3″,90554.

From the ſun, at the mean diſtance of the earth, 1′ 14″,5246.

Compared to that of the earth as 4,31769 to 1.

This planet in bulk is 80,49256 times as large as the earth.

Its denſity as ,220401 to 1.

Its quantity of matter 17,740612 to 1.

And heavy bodies fall on its ſurface 18 feet 8 inches in one ſecond of time.

W. HERSCHEL.

Slough, March 1, 1788.

Fig. 1.

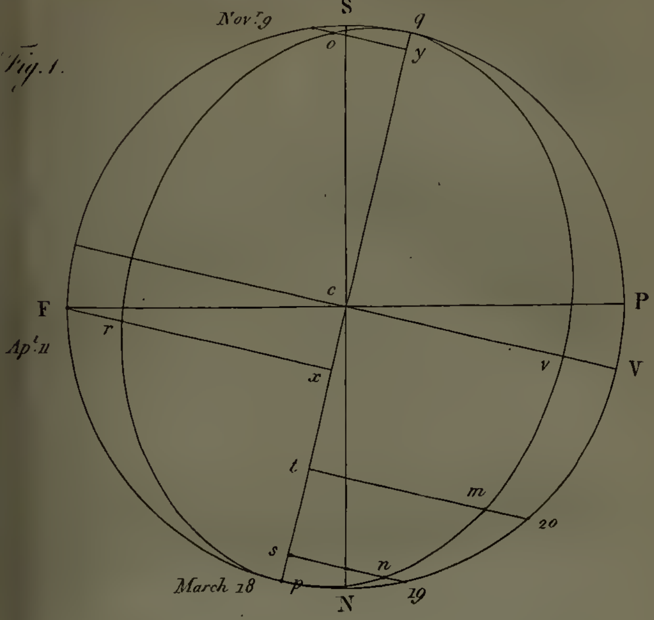


Fig. 2.

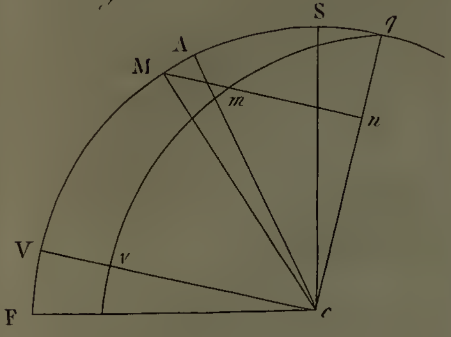


Fig. 3.

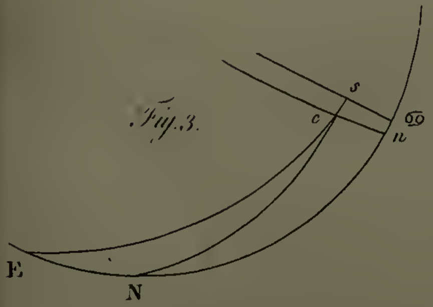
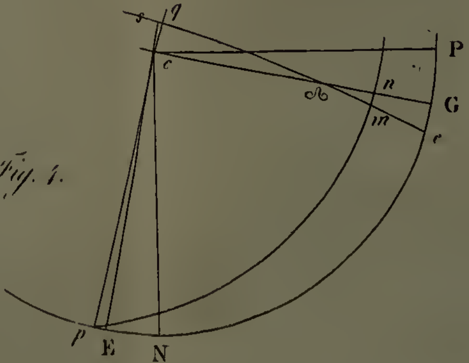


Fig. 4.



XXIII. *Experiments on the Formation of Volatile Alkali, and on the Affinities of the Phlogiſticated and light Inflammable Airs.*

By William Auſtin, *M. D. Fellow of the College of Phyſicians; communicated by* Charles Blagden, *M. D. Sec. R. S.*

Read May 29, 1787.

IN the former part of the year 1787, I undertook to examine the elaſtic fluid produced upon decompoſing volatile alkali by the electric ſtroke, as firſt ſuggeſted by Dr. PRIESTLEY. Some alkaline air being thus decompoſed, and all its inflammable part ſeparated by combuſtion in glaſs veſſels inverted in quickſilver, I obſerved a conſiderable remainder of phlogiſticated air; and after many accurate experiments was fully convinced, that this phlogiſticated air had made a part in the conſtitution of the alkali. This diſcovery induced me to make a variety of ſynthetical experiments on the phlogiſticated and light inflammable airs, with the hopes of forming volatile alkali from its ſimple elements. In this undertaking I alſo derived much aſſiſtance from the ingenious labours of Dr. PRIESTLEY; who, in the courſe of his experiments, had been repeatedly ſtruck with the ſmell of volatile alkali from ſubſtances not ſuppoſed to contain it, and had pointed out ſome important phænomena attending its production. But having acquired a more perfect knowledge of its conſtituent parts, I was enabled to produce volatile alkali in a more ſimple manner, and more demonſtrative of its elements, than he had done.

I was not appriſed, when I had theſe experiments in hand, that the firſt object of my inquiry, the decompoſition of volatile alkali, and analyſis of its parts, had been ſucceſsfully treated by the celebrated M. BERTHOLLET; and that his obſervations had appeared in ROZIER's Journal ſome months before. To him therefore I give up that part of the ſubject; and ſhall confine myſelf in the following obſervations to ſome experiments on the formation of volatile alkali, and on the affinities of the phlogiſticated and light inflammable airs, which have not, I believe, been hitherto taken notice of.

Firſt, I endeavoured to combine the phlogiſticated and light inflammable airs, by mixing them together in various proportions in their elaſtic ſtate, and adding to them ſuch ſubſtances as I thought likely to promote their uniting and forming an alkali. With this view, I threw up to the mixture of theſe airs, marine acid air, the marine and vitriolic acids, to which I alſo joined alkaline air. I tried the effect of cold upon theſe mixtures, by applying to the tubes containing them cloths moiſtened with ether. I even paſſed the electric ſpark repeatedly through them, though with little probability of ſucceſs. Laſtly, I decompoſed alkaline air, and tried to reunite the identical parts which formed it by ſimilar additions; but I could not perceive, that in any inſtance volatile alkali was produced from its two conſtituent parts mixed together in their ſimple aëriform ſtate.

Yet it is well known, that theſe two bodies unite very readily, when they are not in an elaſtic ſtate. An unexpected appearance of volatile alkali had been obſerved by Dr. PRIESTLEY and Mr. KIRWAN before we were acquainted with its conſtitution, and by M. HAUSSMAN ſince this diſcovery of M. BERTHOLLET. An experiment was exhibited before ſeveral Gentlemen at Sir JOSEPH BANKS's Houſe, ſome years ago, in which the quantity of volatile alkali produced is very remarkable. In this experiment a few ounces of powdered tin are moiſtened with ſome moderately ſtrong nitrous acid, and after they have ſtood together a minute or two, about half an ounce of fixed alkali is mixed with them. A very pungent ſmell of volatile alkali is immediately perceived. The experiment ſucceeds equally, if lime be uſed inſtead of fixed alkali. Any perſon, who moiſtens a drachm or two of filings of zinc with a ſolution of cupreous nitre, and, after they begin to act on each other, adds to them a little ſalt of tartar, will find volatile alkali to be produced. Nitrous acid, or cupreous nitre, mixed with iron filings, ſulphur, and a little water, and kept in a cloſe veſſel for ſome hours, yields a ſmell of volatile alkali; and if a piece of paper, ſtained with a vegetable blue ſubſtance, be thrown into the veſſel, it will, in a ſhort time, be turned to a green colour. In each of theſe experiments the nitrous acid and the water are decompoſed. Dephlogiſticated air from each of them combines with the metal, and their other conſtituent parts, the phlogiſticated air of the acid, and inflammable air of the water, being diſengaged at the ſame inſtant, unite and form volatile alkali. Many other ſimilar experiments might be mentioned; but theſe are abundantly ſufficient to prove, that if phlogiſticated and light inflammable air be preſented to each other at the inſtant of their ſeparation from ſolid or liquid ſubſtances, and before their particles have receded from each other, they readily combine and generate volatile alkali.

That theſe two ſubſtances do not combine in their elaſtic ſtate, ſeems to be owing principally to the inflammable air. When theſe two airs combine, it ſeems neceſſary that they part with a certain quantity of that fire to which they owe their elaſticity; and that, unleſs their attraction to each other exceed their attraction to fire, they will not unite. Even when they are combined in the form of volatile alkali, if heat be applied, they immediately recede from each other, and the alkali is decompoſed. When they are not in an aëriform ſtate their attraction to each other is greater, on account of the proximity of their parts; it is then ſuperior to their attraction to fire, and therefore they combine; but when their particles have receded from each other, as in the aëriform ſtate, their attraction to each other is ſo diminiſhed by the diſtance of their parts, that their attraction to fire, which is uniform, prevails, and keeps them in a ſeparate ſtate. The ſpecific gravity of inflammable air being eleven times leſs than that of phlogiſticated air, the diſtance of its particles muſt be greater than the diſtance of the particles of phlogiſticated air in the proportion of $\sqrt[3]{11}$ to 1, if the elementary particles of the two airs be of equal magnitude; and its effect, on this account, in diminiſhing attraction muſt be greater than that of phlogiſticated air, in the proportion of, or more probably as the ſquares of, thoſe numbers.

Whether it be admitted, that fire thus combines with other ſubſtances, and is ſeparated from them by their mutual attractions, according to the general law, is not further material to the preſent enquiry, than as it accounts for a circumſtance which ſeems to be eſtabliſhed by the following experiments; namely, that the combination of the phlogiſticated and inflammable airs, and the formation of volatile alkali, depends chiefly, if not altogether, on the approximation of the parts of inflammable air, when phlogiſticated air is preſented to them.

Into a cylindrical glaſs tube, filled with, and inverted in, quickſilver, I introduced ſome phlogiſticated air, and afterwards ſome iron filings moiſtened with diſtilled water. By this arrangement light inflammable air, which is given out from water in contact with iron filings, meeting with phlogiſticated air at the inſtant of its extrication, combines with it, and forms volatile alkali. In order to detect the minute quantities of volatile alkali, which were thus generated, I fixed to the inſide of the glaſs tube a ſmall piece of paper, ſtained with the rind of the blue raddiſh. The vegetable blue was in twenty-four hours changed to a green colour. As an additional proof of the production of volatile alkali, I kept in the ſame tube ſome paper, which had been dipped in a ſolution of cupreous nitre, expecting to ſee its colour converted from green to blue, by the alkali which was to be produced. The green paper became gradually paler, and in a few days the blue colour appeared. This experiment affords a very ſatisfactory demonſtration of the formation of volatile alkali. Water and iron filings mixed together yield inflammable air; but if this be given out in contact with phlogiſticated air, volatile alkali is produced. In theſe circumſtances a double attraction takes place: one part of the water is attracted by the iron; the other is attracted by the phlogiſticated air; and the water ſeems by theſe compound affinities to be much more rapidly decompoſed, than when iron and water are mixed by themſelves.

Volatile alkali is formed in a very few hours, if nitrous air be uſed inſtead of the phlogiſticated, all other circumſtances remaining as in the former experiment. When I have made uſe of nitrous air not well freed from its acid, by which the vegetable blue colour has been turned red, a ſufficient quantity of alkali has been generated in twenty-four hours to change it to a green. If iron filings and water be expoſed to nitrous air for a conſiderable time, the nitrous air is ſo altered that a candle burns in it with increaſed brightneſs, as was obſerved by

Dr. Priestley. This change is accounted for by the formation of the alkali, which depriving the nitrous air of its phlogiſticated part leaves a greater proportion of dephlogiſticated air.

This experiment alſo ſucceeds in atmoſpheric air, though a longer time is neceſſary to produce a ſenſible alteration in the colours employed as teſts of the alkali; but the change is very evident in a day or two. Hence we may conclude, that whenever iron ruſts in contact with water in the open air, or in the earth, volatile alkali is formed. Phlogiſticated air is preſent in all parts of the terraqueous globe, and operations are conſtantly going on, by which inflammable air is ſeparated from water, and perhaps from other bodies. Thus we may account for the frequent appearance of volatile alkali in the earth, particularly where inflammable matters abound, among coals and volcanic productions, as alſo in animal and vegetable ſubſtances.

When iron, water, and ſulphur act upon each other in atmoſpheric air, volatile alkali is produced. The eudiometer recommended by Scheele is, for this reaſon, incorrect. Some phlogiſticated air diſappears, and volatile alkali is formed. This method therefore ſeems to have miſled that great chemiſt in his analyſis of the atmoſphere, and induced him to ſuppoſe, that the quantity of phlogiſticated air in the atmoſphere is only $2\frac{2}{3}$ times that of dephlogiſticated air.

There is a combination of light inflammable air with ſulphur forming hepatic air. It has been obſerved by the celebrated Mr. Kirwan, that if nitrous air be mixed with hepatic air volatile alkali will be formed. I have often repeated this experiment, and marked the formation of the volatile alkali by the change of the vegetable blue to a green colour. In hepatic air the parts of inflammable air are brought nearer to each other than they are in their ſimple aëriform ſtate*, and therefore the phlogiſticated air of the nitrous air combines with them, and generates volatile alkali.

From all theſe experiments it follows, that whether phlogiſticated air be in a ſtate of purity, or mixed with dephlogiſticated air, as in the atmoſphere, or combined with it as in nitrous air, it will in either caſe unite with the gravitating matter of light inflammable air, provided this ſubſtance be preſented to it in a ſtate of condenſation; but if the circumſtances be reverſed, the ſame combination does not take place. No union is formed between inflammable air and the phlogiſticated part of nitrous air, even though marine acid be added, which, by its attraction to dephlogiſticated air, would contribute to decompoſe the nitrous air, and by its attraction to volatile alkali would tend to unite its conſtituent parts: or if to light inflammable air we add nitrous air and iron filings, no combination enſues; though it has been often obſerved, that volatile alkali is readily generated, when nitrous air is preſented to the inflammable at the inſtant of its extrication from water and iron.

The proportions of the phlogiſticated and inflammable airs in volatile alkali, as diſcovered by calculation, approach very near to the reſult of M, BERTHOLLET's experiments. If we take the ſpecific gravities of theſe airs, given in Mr. KIRWAN's late publication,

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* Since theſe experiments were made, I have found that this is not the caſe. The electric ſpark decompoſes hepatic air, and leaves a quantity of inflammable air equal in bulk to the hepatic air very nearly. However, as the inflammable air leaves the ſulphur upon the application of the electrical ſpark, it ſhould ſeem, that the proper matter of inflammable air is more diſpoſed to combine with fire than with ſulphur; which may be the reaſon why hepatic air is decompoſed by nitrous air, while pure inflammable air is not affected by it.

100 cubic inches contain 18,16 grains of alkaline air.
—— —— —— 30,535 — of phlogiſticated air.
—— —— —— 2,613 — of inflammable air.

According to M. BERTHOLLET alkaline air is expanded upon decompoſitiom from 1,7 to 3,3. Its ſpecific gravity after decompoſition muſt therefore be leſſened in the ſame proportion; and 100 cubic inches will be found to contain only 9,355 grains of alkaline air thus expanded. In what proportion muſt the phlogiſticated and inflammable airs be, in order to form a mixture of this ſpecific gravity?

Let x repreſent the number of grains of phlogiſticated air in 100 cubic inches of the mixture: then $9,355-x$ will expreſs the number of grains of inflammable air. As the weight of one cubic inch is to a cubic inch, ſo will the weight of either air in the mixture be to the cubic inches of that air in the mixture; and therefore $\frac{30,535}{100}$ the weight of a cubic inch of phlogiſticated air, or which is the ſame, ,30535 ſhall be to 1, as x is to $\frac{x}{,30535}$ which muſt be the number of cubic inches of phlogiſticated air in 100 cubic inches of the mixture; and the weight of a cubic inch of inflammable air, that is, $\frac{2,613}{100}$, or $,02613 : 1 :: 9,355-x : \frac{9,355-x}{,02613}$ the cubic inches of inflammable air in 100 cubic inches of the mixture. Thus we have an expreſſion for the cubic inches of each air; theſe two quantities taken together are equal to 100 cubic inches by ſuppoſition. We have then,

$$\frac{x}{{,}30535} + \frac{9{,}355 - x}{{,}02013} = 100;$$

$$x + \frac{2{,}85654925 - {,}30535\,x}{{,}02613} = 30{,}535;$$

$${,}02613\,x + 2{,}85654925 - {,}30535\,x = {,}79787955;$$

$${,}27922\,x = 2{,}05866970$$

$x = 7{,}373$ the number of grains of phlogiſticated air in 100 cubic inches, or in 9,355 grains of the mixture; and 9,355 − 7,373 = 1,982, the grains of inflammable air.

Now 7,373 : 1,982 :: 121 : 32; the quantity of phlogiſticated air is to that of inflammable, as 121 to 32.

According to M. BERTHOLLET's experiments, the quantity of phlogiſticated is to that of inflammable air, as 121 : 29. This is not very wide of calculation. If we conſider the great difficulty of obtaining theſe ſpecific gravities with exactneſs, we muſt be pleaſed to find ſo near a concurrence, and place more confidence in experiments on the ſpecific gravities and combinations of aëriform bodies, than has generally been given them. M. BERTHOLLET's experiments come within $\frac{1}{10}$ of calculation; and this difference will be diminiſhed by two-thirds, if we take the ſpecific gravities of the phlogiſticated and inflammable airs in the proportion of 11 to 1, as he has done, inſtead of Mr. KIRWAN's proportion, which I have followed in this calculation.

XXIV. *Some Properties of the Sum of the Diviſors of Numbers.* *By* Edward Waring, *M. D. F. R. S.*

Read May 29, 1788.

1. LET the equation $\overline{x-1} \,.\, \overline{x^2-1} \,.\, \overline{x^3-1} \,.\, \overline{x-1} \,.\, \overline{x^5-1} \,.\, \ldots \overline{x^n-1} = x^b - px^{b-1} + qx^{b-2} - rx^{b-3} + sx^{b-4} - \&c. = x^b - x^{b-1} - x^{b-2} + x^{b-5} + x^{b-7} - x^{b-12} - x^{b-15} + x^{b-22} + x^{b-26} - x^{b-35} - x^{b-40} + x^{b-51} + x^{b-57} - \&c. \ldots x^{b-n} \pm \&c. = A = 0$. The ſigns + and − proceed alternately by pairs unto the term x^{b-n}. The co-efficients of all the terms to the above mentioned (x^{b-n}) will be +1, −1 or 0; they will be +1, when multiplied into x^{b-v}, where $v = \frac{3z^2+z}{2}$ or $= \frac{3z^2-z}{2}$, and z an even number; but −1, if z be an uneven number; in all other caſes they will be = 0.

The numbers 1, 2, 5, 7, 12, 15, 22, 26, 35, 40, &c. ſubtracted from b may be collected from the addition of the numbers 1, 1, 3, 2, 5, 3, 7, 4, 9, 5, 11, 6, &c. which conſiſt of two arithmetical ſerieſes 1, 3, 5, 7, 9, 11, &c. 1, 2, 3, 4, 5, 6, 7, &c. intermixed.

2. The ſum of any power (m) of each of the roots in the equation $A = 0$ will be $S(m)$, where $S(m)$ denotes the ſum of all the diviſors of the number m, if m be not greater than n.

Cor. Hence (by the rule for finding the ſum of (m) powers of each of the roots from the ſum of the inferior powers and co-efficients of the given equation) may be deduced $S(m) =$

$pS(m-1) - qS(m-2) + rS(m-3) - sS(m-4) + tS(m-5) -$ &c $= S(m-1) + S(m-2) - S(m-5) - S(m-7) + S(m-12) + S(m-15) - S(m-22) - S(m-26) +$ &c. which is the property of the ſum of diviſors invented by the late M. EULER.

Cor. By ſubſtituting for $S(m-1)$, $S(m-2)$, &c. their values $S(m-2) + S(m-3) - S(m-6) - S(m-8) +$ &c., $S(m-3) + S(m-4) - S(m-7) - S(m-9) +$ &c. &c. in the given equation $S(m) = S(m-1) + S(m-2) - S(m-5) - S(m-7) +$ &c. may be acquired an expreſſion for the ſum $S(m)$ in terms of the ſums of the diviſors of numbers leſs than $m-1$, $m-2$, &c. – the ſame method may be uſed for a ſimilar purpoſe in ſome of the following propoſitions.

Cor. By the rule for finding the ſum of the contents of every (m) roots from the ſums of the powers of each of the roots may be deduced the equation $\pm 1 . 2 . 3 . 4 \ldots m$, or

$$v = 1 - m . \frac{m-1}{2} S(2) + m . \overline{m-1} . \frac{m-2}{3} S(3)$$

$$- m . \overline{m-1} . \overline{m-2} . \frac{m-3}{4} S(4) + \&c.$$

$$+ m . \overline{m-1} . \frac{m-2}{2} . \frac{m-3}{2^2} S((2))^2 - \&c.$$

in which the ſum of the diviſors of any number m is expreſſed by the ſums of the diviſors of the inferior numbers $m-1$, $m-2$, &c. and their powers. If v be an even number, then $\pm 1 . 2 . 3 . . m$ will have the ſame ſign as the co-efficient; if uneven, the contrary; but if the co-efficient $= 0$, then will the content $1 . 2 . 3 . . m$ vaniſh. The law of this ſeries is given in the Meditationes Algebraicæ.

3. Let H be the number of different ways by which the ſum of any two numbers 1, 2, 3, 4, . . . $m-2$, $m-1$, can become $= m$; H′ the number of ways by which the ſum of any three of the above-mentioned numbers can make m; H″, H‴, H⁗, &c. the number of ways by which the ſum of any four, five, ſix,

&c.

&c. of the above-mentioned numbers is $= m$ reſpectively; then will $1 - H + H' - H'' + H''' - \&c. = \pm 1$ or 0. Let $m = \frac{3z^2 \pm z}{2}$, and it will be $+1$ or -1, according as z is an odd or even number, in all other caſes it will be $= 0$.

PART II.

1. Let the equation be $\overline{x-1} \,.\, \overline{x^2-1} \,.\, \overline{x^3-1} \,.\, \overline{x^5-1} \,.\, \overline{x^7-1} \,.\, \overline{x^{11}-1} \,.\, \overline{x^{13}-1} \,.\, \overline{x^{17}-1} \ldots \overline{x^n-1} \,.\, \&c. = x^{b'} - px^{b'-1} + qx^{b'-2} - rx^{b'-3} + sx^{b-4} - \&c. = x^{b'} - x^{b'-1} - x^{b'-2} + x^{b'-4} + x^{b'-8} - x^{b'-10} - x^{b'-11} + x^{b'-12} + x^{b'-16} - x^{b'-17} - x^{b'-19} + x^{b'-20} - x^{b'-23} + 2x^{b'-24} - x^{b'-26} - x^{b'-27} + x^{b'-28}, \&c. = A' = 0$; the ſum of any power ($m$) of each of the roots in the equation $A' = 0$ will be $S'(m)$, where $S'(m)$ denotes the ſum of all the prime diviſors of the number m, and m is not greater than n.

Cor. Hence, by the rule before-mentioned $S'(m) = S'(m-1) + S'(m-2) - S'(m-4) - S'(m-8) + S'(m-10) + S'(m-11) - S'(m-12) - S'(m-16) + S'(m-17) + S'(m-19) - S'(m-20) + S'(m-23) - 2S'(m-24) + S'(m-26) + S'(m-27) - S'(m-28) + S'(m-29)$, &c.

If in this, or the preceding, or ſubſequent analogous caſes $S(m-r)$, or $S'(m-r)$, or $S^{l}(m-r)$, becomes $S(o)$, or $S'(o)$, or $S^{l}(o)$; for $S(o)$, or $S'(o)$, or $S^{l}(o)$, always ſubſtitute r.

Cor. Let L be the co-efficient of the term $x^{b'-m}$; then, by the above-mentioned ſeries contained in the Meditationes Algebraicæ, will

$$1 \,.\, 2 \,.\, 3 \,.\, 4 \ldots m \times L = 1 - m \,.\, \frac{m-1}{2} S'(2) + m \,.\, \overline{m-1} \,.\, \frac{m-2}{3} \times S'(3) - m \,.\, \overline{m-1} \,.\, \overline{m-2} \,.\, \frac{m-3}{4} \times S'(4) + \&c. \qquad + m \,.\, \overline{m-1} \,.\, \overline{m-2} \,.\, \frac{m-3}{8} \times S'((2))^2 - \&c.$$

− &c. be an equation, which expreſſes a relation between the prime diviſors of the numbers 1, 2, 3, 4 . . . $m-1$, m, and their powers.

Cor. The co-efficient $L =$ the difference between the two reſpective numbers of different ways that m can be formed adding the prime numbers 1, 2, 3, 5, 7, 11, 13, 19, &c. the one with, and the other without, 2.

PART III.

1. Let an equation $\overline{x^{\alpha}-1} \,.\, \overline{x^{\beta}-1} \,.\, \overline{x^{\gamma}-1} \,.\, \overline{x^{\delta}-1} \times \&c. = x^{b} - px^{b-1} + qx^{b-2} - rx^{b-3} + \&c. = 0$; then will the ſum of the (m) powers of each of its roots be the ſum of all the diviſors of m, that can be found amongſt the numbers α, β, γ, δ, &c.

2. The co-efficient of the term x^{b-m} will be the difference between the two reſpective numbers of different ways, that the number (m) can be formed from the addition of the numbers α, β, γ, δ, &c.; the one containing in it an odd number of the even numbers contained in α, β, γ, δ, &c.; the other not.

PART IV.

1. Let $\overline{x^{l}-1} \,.\, \overline{x^{2l}-1} \,.\, \overline{x^{3l}-1} \,.\, \overline{x^{4l}-1} \,.\,.\,.\,.\,.\, \overline{x^{nl}-1} \,.\, \&c. = x^{b} - px^{b-l} + qx^{b-2l} - rx^{b-3l} + \&c. = x^{b} - x^{b-l} - x^{b-2l} + x^{b-5l} + x^{b-7l} - x^{b-12l} - x^{b-15l} + \&c. = B = 0$, of which equation all the co-efficients are the ſame as in caſe the firſt, and conſequently ± 1 or 0 to the term (x^{b-nl}).

2. The ſum of any power $l \times m$ of each of the roots of the equation $B = 0$ will be $S^{l}(m)$, where $S^{l}(m)$ denotes the ſum of the diviſors of m, which are diviſible by l.

Cor. Hence $S^l(m) = S^l(m-l) + S^l(m-2l) - S^l(m-5l) - S^l(m-7l) + S^l(m-12l) + S^l(m-15l) - S^l(m-22l) - S^l(m-26l) + \&c.$; the law of the ſeries has been given in Caſe 1.

Cor. The ſum of all the diviſors of m not diviſible by $l = S(m) - S^l(m) = S(m-1) - S^l(m-l) + (S(m-2) - S^l(m-2l)) - (S(m-5) - S^l(m-5l)) - (S(m-7) - S^l(m-7l)) + \&c.$

A ſimilar rule may be predicated of the ſum of the diviſors not diviſible by the numbers a, b, c, d, &c: for the ſum of the diviſors of the number (m) diviſible by a, b, c, d, e, &c., where a, b, c, d, e, &c. are prime to each other $= (S^a(m) + S^b(m) + S^c(m) + S^d(m) + S^e(m) + \&c.)) - ((S^{a\times b}(m) + S^{a\times c}(m) + S^{b\times c}(m) + S^{a\times d}(m) + S^{b\times d}(m) + S^{c\times d}(m) + S^{a\times e}(m) + \&c.)) + (S^{a\times b\times c}(m) + S^{a\times b\times d}(m) + S^{a\times b\times d}(m) + S^{b\times c\times d}(m) + S^{a+b+e}(m) + \&c.)) - ((S^{a+b+c+d}(m) + S^{a+b+c+e}(m) + \&c.)) + ((S^{a+b+c+d+e}(m) + \&c.)) - \&c. = l =$ the ſum of all the diviſors of m . . . . diviſible by a, b, c, d, e, &c. reſpectively added together, − the ſum of all the diviſors of m diviſible by the products (ab, ac, bc, &c.) of any two of the quantities a, b, c, d, &c. + the ſum of all the diviſors of m diviſible by the contents (abc, abd, acd, bcd, &c.) of every three of the quantities a, b, c, d, &c. − the ſum of all the diviſors of m diviſible by the contents of every four of the abovementioned quantities a, b, c, d, &c. + and ſo on, and conſequently $S(m) - C$ is the ſum required.

The principles given in the former parts may be applied to this, and extended to equations of which the factors have the formula $x^a \pm k$; and from the ſum of the inferior powers of each of the roots, and the co-efficients may be collected the ſum of the ſuperior; the ſame may be performed by the co-efficients only, &c.

PART

PART V.

1. $S(\alpha \times \beta) = \alpha \times S(\beta)$ + ſum of all the diviſors of β not diviſible by $\alpha = \beta \times S(\alpha)$ + ſum of all the diviſors of α not diviſible by β.

2. $S'(\alpha \times \beta) = \alpha \times S'(\beta)$ + ſum of all the diviſors of β diviſible by l but not by α = &c.

3. $S(\alpha \times \beta \times \gamma \times \delta \times \&c.) = \alpha \times S(\beta \times \gamma \times \delta \times \varepsilon, \&c.)$ + ſum of all the diviſors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not diviſible by $\alpha = \alpha \times \beta \times S(\gamma \times \delta \times \varepsilon, \&c.)$ + ſum of all the diviſors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not diviſible by $\alpha + \alpha \times$ ſum of all the diviſors of $\gamma \times \delta \times \varepsilon$, &c. not diviſible by $\beta = \alpha \times \beta \times \gamma \times S(\delta \times \varepsilon, \&c.)$ + ſum of all the diviſors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not diviſible by $\alpha + \alpha \times$ ſum of all the diviſors of $\gamma \times \delta \times \varepsilon$, &c. not diviſible by $\beta + \alpha \times \beta \times$ ſum of all the diviſors of $\delta \times \varepsilon$, &c. not diviſible by $\gamma = \alpha \times \beta \times \gamma \times \delta \times S(\varepsilon, \&c.)$ + ſum of all the diviſors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not diviſible by $\alpha + \alpha \times$ ſum of all the diviſors of $\gamma\delta\varepsilon$, &c. not diviſible by $\beta + \alpha \times \beta \times$ ſum of all the diviſors of $\delta\varepsilon$, &c. not diviſible by $\gamma + \alpha \times \beta \times \gamma \times$ ſum of all the diviſors of ε, &c. not diviſible by δ = &c. The law of the ſeries is manifeſt. The letters α, β, γ, δ, &c. which are not contained between the parentheſes, denote prime numbers.

Cor. If ſome of the letters α, β, γ, δ, &c. be ſubſtituted for others, and others for them, the equations reſulting will be juſt, and conſequently many new equations may be deduced.

If in the preceding equations for S be wrote S', and for the ſum of all the diviſors of a certain quantity not diviſible by a prime number (α, or β, or γ, &c.) be wrote the ſum of all the diviſors of that quantity not diviſible by the ſame prime number, but diviſible by l; the propoſitions reſulting will be true.

Theſe equations may be applied to the equations given in the preceding parts, and from thence many others deduced.

XXV. *Experiments on the Production of artificial Cold. By Mr.* Richard Walker, *Apothecary to the* Radcliffe Infirmary *at* Oxford. *In a Letter* to Henry Cavendiſh, *Eſq. F.R.S. and A.S.*

Read June 5, 1788.

THE Royal Society having been pleaſed to inſert, among their Tranſactions for laſt year, an account of ſome experiments of mine, relating to the production of artificial cold, tranſmitted in a letter from Dr. Beddoes, I am induced to mention a few I have made ſince.

Your zealous attention to this ſubject, under whoſe auſpices this, as well as other branches of natural philoſophy, hath received conſiderable improvement, will, I hope, apologize for the liberty I have taken in addreſſing myſelf to you, eſpecially ſince any new and uſeful facts I may have aſcertained are principally owing to thoſe endeavours your excellent Papers have incited in me.

My moſt powerful frigorific mixture is the following:

Of ſtrong fuming nitrous acid, diluted with water (rain or diſtilled water is beſt) in the proportion of two parts of the former to one of the latter, each by weight, well mixed, and cooled to the temperature of the air, three parts; of vitriolated natron (Glauber's ſalt) four parts; of nitrated ammonia (nitrous ammoniac) three and a half parts, each by weight, reduced ſeparately to fine powder: the powdered vitriolated natron is to be added to the diluted acid, the mixture well ſtirred, and immediately afterward the powdered nitrated ammonia, again ſtirring the mixture: to produce the greateſt effect, the ſalts ſhould be procured as dry and tranſparent as poſſible, and uſed freſhly powdered. Theſe ſeem to be the beſt proportions when the temperature of the air and ingredients is + 50°; as the temperature at ſetting out is higher or lower than this, the quantity of the diluted acid will evidently require to be proportionably diminiſhed or increaſed. This mixture is but little inferior to one made by diſſolving ſnow in nitrous acid, for it ſunk the thermometer from + 32° to − 20°; perhaps it may be poſſible to reduce the ſalts to ſo fine a powder as to make it equal. In this laſt experiment the diluted acid was equal in quantity to the vitriolated natron, being four parts each, the nitrated ammonia three and a half as before. A powder compoſed of muriated ammonia (crude ſal ammoniac) five parts, nitrated kali (nitre) four parts, mixed, may be ſubſtituted in the ſtead of nitrated ammonia, with nearly equal effect, and in the ſame proportion.

Cryſtallized nitrated ammonia, reduced to very fine powder, ſunk the thermometer, during its ſolution in rain-water, forty-eight degrees, from + 56°, the temperature of the air and materials, to + 8°; and when evaporated gently to dryneſs, and finely powdered, it ſunk the thermometer forty-nine degrees, to + 7°, the temperature of the air and materials being as before at + 56°: therefore, in this ſalt (which produces, as appears above, much greater cold during ſolution in water, than any other hitherto known) the water of cryſtallization is not in the leaſt conducive to that effect. I expected, that by diluting the ſtrong nitrous acid to the proper ſtrength with ſnow, inſtead of water, by which its temperature would be much reduced, and then adding the ſalts, a much greater degree of cold might be produced; but, by various diverſified trials, I found but little advantage gained; I ſhall therefore forbear mentioning the particulars. In the courſe of this winter, ſome diluted nitrous acid, in a wide-mouthed phial, was immerſed in a freezing mixture; when cooled to about – 32°, it froze intirely to the conſiſtence of an ointment, when the thermometer ſuddenly roſe to – 2°; on adding ſome ſnow that lay by me, it became again liquid, and the mercury ſunk into the bulb of a thermometer graduated to – 76°: I know not its exact ſtrength; but by the effect imagine it might correſpond nearly with that which is capable of the eaſieſt point of ſpirituous congelation. Cold, I have found, may be produced by the union of ſuch ſalts as on mixing are decompoſed, and become liquid or partially ſo. The mineral alkali produces this effect with all the ammoniacal ſalts; but with nitrated ammonia to a conſiderable degree. The mineral alkali added in powder to nitrous acid, diluted as above, ſunk the thermometer twenty-two degrees only, from 53° (temperature of air and materials) to 31°. This ſalt contains nearly as much water of cryſtallization as vitriolated natron, and produces more cold during ſolution in water than that ſalt. The reaſon why it produces leſs when added to acid than the neutral ſalt does, is perhaps ſufficiently evident. I have obſerved the thermometer to be ſtationary, or even to riſe, during the violent efferveſcence produced on mixing thoſe materials, and to ſink as ſoon as that ceaſed.

Vitriolated natron diſſolved indifferently in rectified ſpirit of wine, and produced neither heat or cold; the diſpoſition to produce cold, during its ſolution, being perhaps exactly counteracted by the tendency which the diſſolved ſalt hath in uniting with the ſpirit to produce heat. Vitriolated magneſia (a ſalt very ſimilar to vitriolated natron) during ſolution in the diluted nitrous acid, produced nearly as much cold as that ſalt: the ſmall difference there is between them, as to this effect, may be owing to the former containing rather leſs water in its cryſtals.

Vitriolated natron, liquified by heat, was ſet to cool; when its temperature was reduced to 70°, it became ſolid, and the thermometer immediately roſe to 88° (eighteen degrees) its freezing point. Does not the quantity of ſenſible heat evolved by this ſalt, in becoming ſolid, indicate its great capacity for heat, in returning to a liquid ſtate, and conſequently account in a great meaſure for its producing ſuch intenſe cold during ſolution in the diluted mineral acids? Two ſalts, vitriolated argillaceous earth (alum) and tartarized natron (Rochelle ſalt), each contain nearly as much water of cryſtallization as vitriolated natron; but produced neither of them any conſiderable effect during ſolution in the diluted nitrous acid; the latter made the thermometer riſe: neither did their temperatures increaſe, like that ſalt, in changing from a liquid to a ſolid ſtate.

From the obvious application of artificial frigorific mixtures to uſeful purpoſes, in hot climates eſpecially, where the inhabitants ſcarcely know by the ſenſe of feeling winter from ſummer, it may not be amiſs to hint at the eaſieſt and moſt œconomical method of uſing them. For moſt intentions, perhaps, the following cheap one may be ſufficient: of ſtrong vitriolic acid, diluted with an equal weight of water, and cooled to the temperature of the air, any quantity; add to this an equal weight of vitriolated natron in powder: this is the proportion when the temperature ſet out with is + 50°, and will ſink the thermometer to 5°; if higher, the quantity of the ſalt muſt be proportionably increaſed. The obvious and beſt method of finding the neceſſary quantity of any ſalt to produce the greateſt effect, by ſolution in any liquid, at any given temperature, is by adding it gradually until the thermometer ceaſes to ſink, ſtirring the mixture all the while.

If a more intenſe cold be required, double aqua fortis, as it is called, may be uſed; vitriolated natron, in powder, added to this, produces very nearly as much cold as when added to the diluted nitrous acid: it requires a rather larger quantity of the ſalt, at the temperature of + 50°, about three parts of the ſalt to two parts of the acid: it will ſink the thermometer from that temperature nearly to 0, and the conſequence of more ſalt being required is, its retaining the cold rather longer. This mixture has one great recommendation, a ſaving of time and trouble. A little water in a phial, immerſed in a ſmall tea-cup of this mixture, will be ſoon frozen in ſummer; and if the ſalt be added in cryſtals unpounded to double aqua fortis, even at a warm temperature, the cold produced will be ſufficient to freeze water or creams; but if diluted with one-fifth its weight of water, and cooled, it is about equal to the diluted nitrous acid above mentioned, and requires the ſame proportion of the ſalt. A mixture of vitriolated natron and diluted nitrous acid ſunk the thermometer from + 70° (temperature of air and ingredients) to + 10°.

The cold in any of theſe mixtures may be kept up a long time by occaſional additions of the ingredients in the proportions mentioned. A chemiſt would make the ſame materials ſerve his purpoſe repeatedly.

Equal parts of muriated ammonia and nitrated kali in powder make a cheap and convenient compoſition for producing cold by ſolution in water; it will, by the following management, freeze water or creams at Midſummer.

June 12th, 1787, a very hot day, I poured four ounces, wine meaſure, of pump-water, at the temperature of 50° (it is well known that water at ſprings retains nearly the ſame temperature winter and ſummer, *viz.* about 50°, to which temperature the water may be reduced during the warmeſt weather, by pumping off ſome firſt) upon three ounces, Avoirdupois weight, of the above powder (previouſly cooled by immerſing the veſſel containing it in other water at 50°), and after ſtirring the mixture its temperature was 14°; ſome water contained in a ſmall phial, immerſed in this mixture, was conſequently ſoon frozen. This ſolution was afterwards evaporated to dryneſs, in an earthen veſſel, reduced to powder, and added to the ſame quantity of water, under the ſame circumſtances as before, when it again ſunk the thermometer to 14°. Since that time I have repeatedly uſed a compoſition of this kind for the purpoſe of producing cold, without obſerving any diminution in its effect after many evaporations. The cold may be œconomically kept up and regulated any length of time, by occaſionally pouring off the clear ſaturated liquor, and adding freſh water, obſerving to ſupply it conſtantly with as much of the powder as it will diſſolve.

The degree of cold at which water begins to freeze has been obſerved to vary much; but that it might be cooled twenty-two degrees below its freezing point was perfectly unknown to me until lately. I filled the bulb of two thermometers, one with the pureſt rain-water I could procure, the other with pump water; the water was then made to boil in each, until one-third only remained: theſe were kept in a frigorific mixture, at the temperature of +10°, for a much longer time than I thought neceſſary to cool the water to the ſame temperature; and by repeated trials I found it was neceſſary to lower the temperature of the mixture to near +5°, to make the water in either of them freeze. Theſe were likewiſe ſuſpended out of doors, cloſe to a thermometer, during the late froſt, and the water never obſerved frozen. On March the 22d, at ſix in the morning, the water in each remained unfrozen, though the tubes were gently ſhaken, the thermometer ſtanding at that time at 23°. There appeared to be little difference with reſpect to the degree of cold neceſſary to freeze the water, whether the tube of the thermometers were open or cloſed in vacuo (which was very nearly effected by ſuffering the water to boil up to the orifice of the tube, and then ſuddenly ſealing it) or not, but unboiled water in the ſame ſituation froze in a higher temperature.

It is commonly ſuppoſed, I believe, that gentle agitation of any kind will diſpoſe water (cooled below its freezing point) to become ice; but I have repeatedly cooled rain-water and pump-water, boiled a long time, and unboiled, in open veſſels to 30° or lower, and have conſtantly ſucceeded, after trying other kinds of agitation in vain, by ſtirring, or rather ſcraping gently, the bottom and ſides of the veſſel containing the water to be frozen, when after ſome ſhort time ſmall filaments of ice appeared, and by continuing this motion about every part of the veſſel beneath the ſurface of the water, about two-thirds of the water commonly froze. A ſlender, pointed glaſs rod I uſed for this purpoſe.

I have the honour to be, &c.

RICH. WALKER.

Oxford, March 27, 1788.

Extract

Extract from a ſecond Letter from Mr. Walker *to* Henry Cavendiſh, *Eſq. Dated* Oxford, May 28, 1788.

A more intenſe cold may be produced by a ſolution of ſalts in water in ſummer, than can be produced by a mixture of ſnow and ſalt in winter. To rain-water ſix drachms (by weight) I added ſix drachms of nitrated ammonia reduced to a very fine powder which made the thermometer ſink from +50° (temperature of the materials) to 4°, then adding ſix drachms of mineral alkali very finely powdered the thermometer ſunk to −7°, fifty-ſeven degrees. It is obſervable, that in the latter there are two cauſes concur in producing the effect, the liquifaction both of the ſnow and ſalt; but in the experiment juſt mentioned the liquifaction of the ſalts only.

Vitriolated natron, after it had given out its water of cryſtallization by expoſure to the atmoſphere, produced no change of temperature by ſolution in the diluted nitrous acid, but during ſolution in water produced heat, as did likewiſe the mineral alkali.

I have ſince my laſt ſeen FAHRENHEIT's Experiments on the freezing of Water, related in Vol. XXXIII. of the Philoſophical Tranſactions; but as mine differ in degree I take no farther notice of them.

XXVI. *A Deſcription of an Inſtrument which, by the turning of a Winch, produces the Two States of Electricity without Friction or Communication with the Earth. In a Letter from Mr.* William Nicholſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 5, 1788.

SIR,

THE following account of the inſtrument I had the honour to ſhew to yourſelf and other philoſophical friends, in the month of March laſt, will, I hope, appear ſufficiently intereſting to be communicated to the learned Society, over which you ſo worthily preſide.

Plate VI. fig. 1. repreſents the apparatus ſupported on a glaſs pillar 6½ inches long. It conſiſts of the following parts. Two fixed plates of braſs, A and C, are ſeparately inſulated and diſpoſed in the ſame plane, ſo that a revolving plate B may paſs very near them, without touching. Each of theſe plates is two inches in diameter; and they have adjuſting pieces behind, which ſerve to place them accurately in the required poſition. D is a braſs ball, likewiſe of two inches diameter, fixed on the extremity of an axis that carries the plate B. Beſides the more eſſential purpoſe this ball is intended to anſwer, it is ſo loaded within on one ſide, that it ſerves as a counterpoiſe to the revolving plate, and enables the axis to remain at reſt in any poſition. The other parts may be diſtinctly ſeen in fig. 2. The ſhaded parts repreſent metal and the white repreſent varniſhed glaſs. ON is a braſs axis, paſſing through the piece M, which laſt ſuſtains the plates A and C. At one extremity is the ball D already mentioned; and the other is prolonged by the addition of a glaſs ſtick, which ſuſtains the handle L and the piece GH ſeparately inſulated. E, F, are pins riſing out of the fixed plates A and C, at unequal diſtances from the axis. The croſs-piece GH, and the piece K, lie in one plane, and have their ends armed with ſmall pieces of harpſichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is likewiſe a pin I, in the piece M, which intercept a ſmall wire proceeding from the revolving plate B.

The touching wires are ſo adjuſted, by bending, that when the revolving plate B is immediately oppoſite the fixed plate A, the croſs-piece GH connects the two fixed plates, at the ſame time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately oppoſite the fixed plate C, the ball becomes connected with this laſt plate, by the touching of the piece K againſt F; the two plates, A and B, having then no connection with any part of the apparatus. In every other poſition the three plates and the ball will be perfectly unconnected with each other.

Mr. CAVALLO's diſcovery, ſo well explained in the laſt BAKERIAN Lecture, that the minute differences of electrization in bodies, whether occaſioned by art or nature, cannot be completely deſtroyed in any definite time, may be applied to explain the action of the preſent inſtrument. When the plates A and B are oppoſite each other, the two fixed plates A and C may be conſidered as one maſs; and the revolving plate B, together with the ball D, will conſtitute another maſs. All the experiments yet made concur to prove, that theſe two maſſes will not poſſeſs the ſame electric ſtate; but that, with reſpect to each other, their electricities will be plus and minus. Theſe ſtates would be ſimple and without any compenſation, if the maſſes were remote from each other; but as that is not the caſe, a part of the redundant electricity will take the form of a charge in the oppoſed plates A and B. From other experiments I find that the effect of the compenſation on plates oppoſed to each other, at the diſtance of one-fortieth part of an inch, is ſuch that they require, to produce a given intenſity, at leaſt one hundred times the quantity of electricity that would have produced it in either, ſingly and apart. The redundant electricities in the maſſes under conſideration will therefore be unequally diſtributed: the plate A will have about ninety-nine parts, and the plate C one; and, for the ſame reaſon, the revolving plate B will have ninety nine parts of the oppoſite electricity, and the ball D one. The rotation, by deſtroying the contacts, preſerves this unequal diſtribution, and carries B from A to C, at the ſame time that the tail K connects the ball with the plate C. In this ſituation, the electricity in B acts upon that in C, and produces the contrary ſtate, by virtue of the communication between C and the ball; which laſt muſt therefore acquire an electricity of the ſame kind with that of the revolving plate. But the rotation again deſtroys the contact, and reſtores B to its firſt ſituation oppoſite A. Here, if we attend to the effect of the whole revolution, we ſhall find that the electric ſtates of the reſpective maſſes have been greatly increaſed: for the ninety-nine parts in A and in B remain, and the one part of electricity in C has been increaſed ſo as nearly to compenſate ninety-nine parts of the oppoſite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A ſecond rotation will, of courſe, produce a proportional augmentation of theſe increaſed quantities; and a continuance of turning will ſoon bring the intenſities to their maximum, which is limited by an exploſion between the plates.

If one of the parts be connected with an electrometer, more eſpecially that of BENNET, theſe effects will be very clearly ſeen. The ſpark is uſually produced by a number of turns between eleven and twenty; and the electrometer is ſenſibly acted upon by ſtill fewer. When one of the parts is occaſionally connected with the earth, or when the adjuſtment of the plates is altered, there are ſome variations in the effects, not difficult to be reduced to the general principles, but ſufficiently curious to excite the meditations of perſons the moſt experienced in this branch of natural philoſophy. An attention to brevity, however, renders it neceſſary to forbear enlarging upon them.

If the ball be connected with the lower part of BENNET's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the poſition of the apparatus is ſuch that the croſs-piece GH touches the two pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity muſt be ſtrong enough to deſtroy and predominate over any other electricity the plates may poſſeſs. I ſcarcely need obſerve, that if this difficulty ſhould hereafter be removed, the inſtrument will have great advantages as a multiplier of electricity in the facility of its uſe, the very ſpeedy manner of its operation, and the unequivocal nature of its reſults.

I have the honour to be, &c.

W. NICHOLSON.

XXVII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain at* Lyndon *in* Rutland; *with the Rain in* Hampſhire *and* Surrey, *in* 1787. *Alſo ſome Account of the Annual Growth of Trees. By* Thomas Barker, *Eſq. Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 12, 1788.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.			Lyndon	Hampſhire.		Surre
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean		Sel-bourn.	Fyfield.	S.La beth
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch
n.	Morn.	30,13	29,11	29,70	45	34½	39	47	25	35	0,415	0,88	0,43	0,6
	Aftern.				46	34½	40	51	31	39½				
b.	Morn.	29,85	28,15	29,40	47	41	44	47½	27	39	0,860	3,67	3,40	1,6
	Aftern				48½	41½	45	53½	37	46				
ar.	Morn.	30,02	28,47	29,30	49	41	45½	49	31	40	1,782	4,28	3,80	1,6
	Aftern.				50½	42	46½	54	35½	48½				
pr.	Morn.	30,00	28,54	29,47	50	44	46½	50½	35½	42	1,721	0,74	0,69	0,9
	Aftern.				50½	45	47½	50½	42½	50				
ay	Morn.	29,86	28,80	29,47	60½	42½	52½	55½	36	48½	1,573	2,06	1,27	1,6
	Aftern.				62	44	54	72½	46½	59				
ne	Morn.	29,76	29,05	29,44	63	51½	58	60½	46	54½	1,800	1,50	1,43	0,68
	Aftern.				64½	53	59½	77	55	65½				
ly	Morn.	29,93	28,92	29,36	69	56½	60½	66	50½	57	3,169	6,53	3,50	4,12
	Aftern.				70	57	62	79	63	68				
ug.	Morn.	29,94	28,74	29,53	70	55½	61	63½	49½	56	1,969	0,83	0,74	0,6
	Aftern.				73	56½	63	80	51½	67½				
pt.	Morn.	30,01	28,59	29,50	60	53½	57	57½	42	51	1,225	1,56	1,47	0,78
	Aftern.				60½	55	57½	65	55	61				
ct.	Morn.	29,70	28,48	29,26	56	46½	51½	55	36	46	3,726	5,04	3,44	2,41
	Aftern.				57	47	53	61	44½	54½				
ov.	Morn.	29,95	28,50	29,33	52	34	43	49	20	35½	1,462	4,09	2,57	1,51
	Aftern.				52½	35½	43	54½	30½	42				
ec.	Morn.	29,93	28,75	29,22	48	34	41	48½	26	37	3,085	5,06	3, 8	3,87
	Aftern.				49	35	41½	55½	30½	41				
										Inches	22,787	36,24	25,82	20,40

The

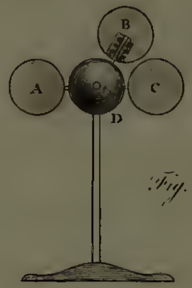

Fig. 1.

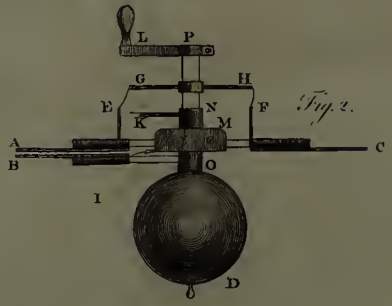

Fig. 2.

The year began dry, with but little and moderate froſt, and proved a pleaſant and open winter: as February approached it grew mild, was fine, forward, and drying, and continued forward all March, but more ſhowery the beginning and end of that month. With April the weather turned colder; eaſt and north winds the firſt three weeks ſtopped vegetation, and after a ſhowery latter end of the month. May had ſo much cloudy weather and cold winds as blaſted the apples, and hindered the growth of graſs; but being dry was not unpleaſant, and ſuited the grain very well, which proved good.

The bees were remarkably forward in drones this year, which they had the middle of April; but the cold winds and cloudy weather afterward ſo hindered their working, that they killed their drones in May, and were backwarder the beginning of June than they had been ſeveral weeks before.

There were ſeveral froſty mornings in May, and one on the 7th of June, ſo ſharp as to kill the young ſhoots on ſome of the oaks and aſh trees in the meadows. The ſeaſon was in general dry the firſt half of the year, and the crops of hay were light. It was a very cool ſummer; what hot weather there was came toward the end of June, the beginning of July, and beginning of Auguſt.

July 6, a very ſhowery ſeaſon began, which continued with but few intermiſſions to the end of the year, and was more remarkable for the frequency than the quantity of rain, which was not very great here. This made great plenty of graſs and turneps; but was very troubleſome in hay-time and harveſt, yet more hindering than real hurting. The wetteſt months were July, October, and December. The chief fair fits were a hot week in Auguſt, and a clear froſty week at the full moon in November. The latter part of the ſummer was very cool, the autumn mild, and not very froſty; but ſome great ſtorms, and in the countries where they had more rain than we, there were great floods. At the full moon in December there came a week's froſt and a large ſnow, which was not quite gone at the end of the year.

On the annual growth of trees.

Oaks.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
1	——	——	1772	19	—	1787	41	1.5
2	1758	13	1772	33	1.4	1787	55½	1.5
3	1758	18½	1772	40½	1.6	1787	58.	1.2
4	——	——	——	——	—	1787	156½	1.3
5	1758	41	1772	56	1.1	1787	77½	1.4
6	1744	20	1765	45	1.2	1787	74½	1.3
7	1758	18	1772	36	1.3	1787	54	1.2
8	1758	76	1772	93½	1.25	1787	109½	1.1
9	1751	124	1772	147	1.1	1787	164½	1.2
10	1744	23½	1765	49	1.2	1787	74	1.1
11	1744	69½	1772	99	1.1	1787	115	1.1
12	1744	14	1765	43	1.4	1787	60	0.8
13	1747	82	1765	99½	1.0	1787	120½	1.0
14	1744	21	1765	45	1.1	1787	64	0.9
15	1762	106½	1772	117	1.0	1787	130	0.9
16	1751	117	1770	132	0.8	1787	149½	1.0
17	1751	114	1770	131½	0.9	1787	145	0.8
18	1751	84½	1772	101	0.8	1787	109	0.5
19	1744	41	1765	58½	0.8	1787	69	0.5

Aſh.

Aſh.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
20	——	——	1772	71	—	1787	106	2.3
21	1745	23½	1765	67	2.2	1787	111	2.0
22	1744	22	1765	55½	1.6	1787	92	1.7
23	1744	32	1765	61	1.4	1787	94½	1.5
24	1744	66	1765	91½	1.4	1787	114	1 0
25	1751	20	1772	45	1.25	1782	58½	0.9
26	1765	55	1772	64	1.2	1787	75½	0.9
27	1747	77	1765	97	1.0	1787	116½	1.0
28	——	——	1772	67½	—	1787	82	1.0
29	1744	56	1765	77½	1.0	——	——	——
30	1755	51½	1772	67	0.9	1787	80	0.9
31	——	——	1765	74	—	1781	89	0.9
32	1751	45½	1772	67	1.0	1787	77	0.7
33	1744	17½	1765	34	0.8	1787	52	0.8
34	1744	17	1765	36½	0.9	1787	52½	0.7
35	1744	20	1772	40	0.7	——	——	——
36	1745	13½	1772	31½	0.7	1787	41	0.6

Elms.

37	1755	0	1772	42	2.5	1787	77	2.3
38	1744	28	1765	60	1.5	1787	96	1.6
39	1744	37	1758	50	0.9	1781	72	1.0
40	1744	46	1758	58	0.9			
41	1744	48	1758	59	0.8			

As I do not remember to have ſeen any particular account of the annual growth of trees, I here ſend an abſtract of ſome obſervations I have made on a conſiderable number, for a good many years back; and have diſtinguiſhed in the table the different kinds, and placed the moſt growing firſt.

Except the two firſt aſh trees, I find the growth of oak and aſh to be nearly the ſame. I have ſome of both ſorts planted at the ſame time, and in the ſame hedges, of which the oaks are the largeſt, but there is no certain rule as to that. The common growth of an oak or an aſh is about an inch in girth in a year; ſome thriving ones will grow an inch and an half; the unthriving ones not ſo much, ſome probably leſs than any here, for I choſe in general to meaſure thoſe that ſeemed thriving.

Great trees grow more timber in a year than ſmall ones; for if the annual growth be an inch, a coat of one-ſixth of an inch is laid on all round, and the timber added to the body every year is its length multiplied into the thickneſs of the coat, and into the girth, and therefore the thicker the tree is, the more timber is added. The body of N° 9 is 9 feet long, the girth under the bark above 13 feet, the thickneſs of the coat $\frac{1}{6}$ of an inch or $\frac{1}{72}$ of a foot: then $9 \times 13 \times \frac{1}{72}$ is one foot and ſix-tenths of timber added in a year to the body, beſide the increaſe on all the branches, and it has a very great head; one limb ſquares 20 inches, and is itſelf equal to a moderate tree.

The hedge in which N° 4. grows was planted in 1665, probably the tree is not older than that year; it has therefore increaſed in girth about 1.3 inch every year ſince it was ſet.

The oak, N° 5. I believe ſowed itſelf; and I did not know there was ſuch an one till about the year 1740, when the hedge being cut, the tree was found, and might be then 20 years old or more.

The two aſh trees N° 20. and 21. grow much faſter than any of the reſt, but are neither of them handſome growing trees. N° 20. has ſeveral ſeams where the bark is parting from the

the wood, and are likely to be dead ſides. N° 21. was about as thick as a walking-ſtick in 1730. It does not grow round and ſmooth, has no dead ſide, but ſeveral deep furrows in it, ſo that theſe two trees ſeem to grow faſter than they can grow well.

In 1733, N° 23, was about as thick as a pitch-fork ſhaft.

The elm, N° 37. was planted with the quick in January, 1756, and cut down to the ground as that was. It is a kind of witch elm, which grow faſter than the upright ones, and with great round heads. N° 38. is ſo far like a witch elm, that at ten feet high it parts into a great head; but it grows much ſtraighter and handſomer than that kind of tree generally does.

Planted trees at a diſtance from the hedge ſeem not to grow ſo large as ſown trees in the hedge: whether from the check the roots receive in tranſplanting, or that the trees not in hedges are more rubbed by the cattle; perhaps both cauſes concur when the trees are tranſplanted large; but trees ſet in quicks, when very ſmall, do not ſeem to be hurt by it. I have ſome oaks ſet with the quick, and a row of acorns was ſome years after ſown againſt it; but in between forty and fifty years they have not overtaken the planted ones in ſize; the ſown ſeem however inclined to be taller trees than the planted.

❧

XXVIII. *On the Era of the Mahometans, called the Hejerà* * (هجرة). *By* William Marſden, *Eſq.* *F. R. S. and A. S.*

Read June 12, 1788.

IN their computation of time, the Arabs, and other Mahometan nations, reckon by a year which is purely lunar. It has no reference to the ſolar revolutions, and is of courſe unconnected with the viciſſitude of ſeaſons. The purpoſe of its adoption appears to have been chiefly religious, for the regulation of faſts and ceremonies, rather than of the civil concerns of the people. Perhaps a conſcious ignorance in matters of ſcience might have determined the inſtitutors to prefer a period whoſe limits were marked and obvious to the ſenſes, to one whoſe ſuperior accuracy depended upon aſtronomical calculation; and it may alſo be conjectured, that their habits of life rendered the adjuſtment of the tropical year leſs intereſting to theſe turbulent and wandering fanatics, than to nations whoſe attention was directed to agriculture and other peaceful arts.

The era of the Mahometans, called by them the Hejerà, or Departure, is accounted from the year of the flight of Maho-

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As this mode of ſpelling the word differs from that commonly followed, it may be proper to obſerve, that the Arabic letters of which it is compoſed are *H*, *j*, *r*, *à* or *ah*, and that the ſupplied vowels are to be pronounced ſhort.

met, their prophet, from Mecca, in Arabia Petræa, to Medina, at that time called Yatreb, which was the thirteenth of his pretended miſſion, the year of Chriſt 622, and of the Julian period 5335. This event, but little memorable in itſelf, and deriving no celebrity from the circumſtances immediately attending it, was, eighteen years after, diſtinguiſhed by the Caliph OMAR, as the criſis of their new religion, and eſtabliſhed as an epoch, to which the dates of all the tranſactions of the faithful ſhould have reference in future (*a*) *. Previous to this, the people had been accuſtomed to compute from the commencement of a particular war, the day of a remarkable battle, or other occaſional event of importance to their little communities (*b*). Accordingly, MAHOMET is ſaid to have been born in the firſt year of the era of the *Elephant*, ſo called from an attack on the city and temple of Mecca, by a king of Abyſſinian race, in which thoſe animals were employed; and twenty years after this, the *impious* war, in which the animoſity of two contending tribes occaſioned them to violate the ſacred or interdicted months, appeared of conſequence ſufficient to give riſe to a new era. The uncertainty and confuſion produced by this fluctuation demanded a reform, and more forcibly in proportion as the intereſts and concerns of the growing empire extended themſelves. A diſpute between two individuals, reſpecting the year in which the term of an obligation for money ſhould be underſtood to expire (the parties being agreed as to the month), pointed out to the Caliph, to whoſe tribunal it was referred, the immediate neceſſity of enjoining the obſervance of a determinate era, in which the ſtrongeſt prejudices of the people ſhould be made to concur with the ſovereign authority. The date of the Hejerà was thenceforth expreſſed in all the public acts and letters.

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* See the authorities at the end, under this and the ſubſequent letters.

It muſt be underſtood, that although the account of the years, collectively conſidered, was vague, that of the months was certain, and their ſucceſſion at all times ſcrupulouſly attended to. Omar did not think it expedient to attempt any innovation as to the time of beginning the year, againſt which the ideas of the people would have revolted; and therefore, although the eſcape of Mahomet from the indignation of his fellow-citizens was effected, according to their records, on the firſt day of the third month, or Rabee prior (on the twelfth day of which he reached Medina), yet the Hejerà takes date from a period two months antecedent to this flight, namely, from the firſt day of Moharram, being the day on which immemorial cuſtom had eſtabliſhed the celebration of the feſtival of the new year (*c*).

The Arabian and Syrian Chriſtians, and the Mahometan aſtronomers in general, appear to have fixed this day to Thurſday the fifteenth of the Syro-Macedonian month Tamooz, anſwering to our July; but ſome among the latter, and moſt of their hiſtorical writers, refer it to the next day, Friday the ſixteenth, and this latter date has, in modern times, obtained almoſt univerſal acceptance (*d*). A religious preference which Friday claims above the reſt of the week, ſeems to have given effect to the arguments in its favour (*e*). The difference of opinion on this ſubject has ariſen, in the firſt place, from the uncertainty unavoidably attending a date, to be aſcertained, at a diſtant period of time, from the phaſe of the moon, which is retarded or advanced by ſo complicated a variety of circumſtances (*f*): and the ambiguity appears, in the ſecond place, to have been promoted by the cuſtom of the

Arabs beginning their day at ſun-ſet; conformably with which idea, the time when the moon became viſible at Mecca, being the evening of Thurſday the fifteenth, according to our mode of computation *, was to them the commencement of Friday; which Friday (beginning a few hours later) we term the ſixteenth of July. At that period the cycle of the ſun was 15; the cycle of the moon, or golden number, 15; the Roman indiction 10; and the dominical letter C.

The year of the Mahometans conſiſts of twelve lunar months, and no emboliſm being employed to adjuſt it to the ſolar period (as practiſed by the Chaldæans and Hebrews, who were in other particulars their guides, and anciently, it is ſaid, by the Arabs themſelves), the commencement of each ſucceſſive lunar year anticipates the completion of the ſolar, and revolves through all its ſeaſons, the months reſpectively preſerving no correſpondence (*g*).

In order to form a juſt and accurate idea of the length of this year, and of its component months, it will be neceſſary to diſtinguiſh two modes of eſtimating their commencement and duration. Theſe, though their difference is not progreſſive (never amounting to more than two whole days, and rarely to ſo much as one), may yet, if miſunderſtood, occaſion, in ſome inſtances, uncertainty and error: and more eſpecially as the writers on this ſubject have inadvertently fallen into contradictions, from neglecting to explain to their readers a diſtinction of which they muſt have been themſelves ſufficiently aware[*]. Theſe modes may be denominated the vulgar or practical, and the political or chronological reckoning.

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* The new moon happened in July 622, on the 14th day, at 5½ hours, A.M. Greenwich time, or about 8 hours Mecca time; and at ſun-ſet of the ſame day, the moon was 5¼ degrees before the ſun in longitude, and in 40 minutes ſouth latitude, and therefore about 4¼ degrees above the horizon. On the 15th, at ſun-ſet, it was 18 ¼ before the ſun in longitude, in 37′ north latitude, and about 15°¼ above the horizon, conſequently viſible with clear weather. The ſun ſets at Mecca, on the 15th July, at 6 h. 40′, and the twilight is there conſiderably ſhorter than in the high latitudes.

The vulgar or practical reckoning is that which eſtimates the commencement of the year, or firſt day of the month Moharram, from the appearance of the new moon, on the evening of the firſt or ſecond day after the conjunction, or from that time at which it might from its age be viſible, if not obſcured by the circumſtances of the weather, which is ſcarcely ever ſo ſoon as twenty-four hours, and ſeldom later than forty-eight hours, after the actual change. This appearance is announced by perſons placed on the pinnacles of the moſques or other elevated ſituations, to the people below, who welcome it with the ſound of inſtruments, firing of guns, and other demonſtrations of reſpect and zeal[†]. The month thus commenced is computed to laſt till the new moon again becomes viſible; and ſo of the remaining months, till ſhe has completed her twelfth lunation, and, emerging from the ſun's rays, marks the practical commencement of another year (*h*).

In the political or chronological mode of reckoning, the return of a new year, or the duration of the months which compoſe it, is not regulated either by the appearance of the moon, or the calculated period of conjunction, but according to a certain diviſion of a cycle of thirty years, adopted for this purpoſe.

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[*] The juſtice of this remark will appear evident by contraſting the authorities quoted under the letters (*h*) (*i*) and (*m*).

[†] Theſe ſalutations are more ſolemn or clamourous at the return of ſome months than of others, and particularly on the appearance which terminates the month of faſting, or Ramadan.

purpoſe *. Particular attention is due to the explanation of this mode, both as being more artificial and complex, and becauſe it ſerves to regulate the dates in matters of hiſtorical record, and indeed of all writings where pretenſion is made to accuracy (*k*). Upon this the Turkiſh, Mooriſh, and every ſyſtematic Mahometan calendar are founded.

The lunar month, or mean ſynodic revolution, according to the computation of the Arabian aſtronomers, conſiſts of 29 days, 12 hours, and 792 ſcruples or parts in 1080; and the year of 354 days, 8 hours, and 864 ſcruples. But, as the purpoſes of mankind require that the year ſhould contain an integral number of days, it became expedient to collect and diſpoſe of theſe fractional exceedings in a conſiſtent and practicable manner; and with this view, a cycle or period of thirty lunar years was choſen, as the loweſt number that admitted of their being formed into days, without ſenſible deficiency or remainder. Their ſum being 11 days, it was determined that 19 of thoſe thirty years ſhould be compoſed of 354 days, and 11 of 355 days each. The juſtneſs of this proportion will equally appear, if it be obſerved, that 8 hours and 864 ſcruples (or 48 minutes) conſtitute 11 parts in 30 of twenty-four hours, and conſequently in thirty years produce an exceſs of 11 whole days †. It remained next to be conſidered in what order and method theſe additional or intercalary days ſhould be inſerted, ſo as to affect the compenſation required with as much equability as poſſible, and maintain a correſpondence, as near as circumſtances would admit, with the periods marked by the phaſes of the moon. The following are the years to which, for reaſons that ſhall be afterwards aſſigned, it was judged proper to annex an extraordinary day, and which (in contradiſtinction to thoſe 19 that have only 354 days) are termed *years of exceſs*, viz. the 2d, 5th, 7th, 10th, 13th, 16th, 18th, 21ſt, 24th, 26th, and 29th, of the cycle of thirty years.

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* A paſſage in ALFRAGANUS (who wrote about the year of Chriſt 950) would lead us to infer, that, beſide the two ways of computing time here diſtinguiſhed, the Aſtronomers were accuſtomed to follow a third, whoſe periods were marked by the conjunction of the luminaries: but, as this learned Mahometan was a profeſſed ſtudent of PTOLEMY's works (which in this place he quotes), we may conclude, that, when he ſpeaks of aſtronomers, he does not mean to confine the expreſſion to thoſe of his own country or religion (*i*).

† The mean ſynodic revolution being 29 d. 12 h. 44′ and nearly 3″, this cycle falls

Their months, conformably with thoſe of the Hebrew calendar, it was determined ſhould conſiſt alternately of 30 and 29 days; and therefore, in an ordinary or ſimple year of 354 days, the twelfth and laſt month, Dulhajee, would have only 29; but, in the years of exceſs, the intercalary day is added to this month, which is then made to conſiſt of 30 days, and the year, conſequently, of 355 days (*m*). Thus, for example, in the year of Chriſt 622, the Hejerà commenced on the 16th of July, with the Arabian month

	Days.		Days.
Moharram, which had	30	Moharram, in the 2d year, had	30
Safar - - -	29	Safar - - -	29
Rabee prior - -	30	Rabee prior - -	30
Carried over	89	Carried over	89

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falls ſhort of thirty complete lunar years, by ſomething more than 17, and conſequently advances one day in about 2500 years. The Chaldæans, who made the time of the revolution to conſiſt of one ſcruple, or 1080th part of an hour, more than the Arabs thought fit to allow, were wonderfully near to the truth (*l*).

If, inſtead of thirty years, a cycle of nineteen had been choſen, and ſeven days intercalated, there would have been an exceſs of a thirtieth part of a day, which would have cauſed the reckoning to retrograde one day in 570 years.

	Days.
Brought over	89
Rabee poſterior -	29
Joomad prior - -	30
Joomad poſterior -	29
Rajab - - -	30
Saban - - -	29
Ramadan - -	30
Sawal - - -	29
Dulkaidat - -	30
Dulhajee - -	29
Days	354

Ended 5 July 623.

	Days.
Brought over	89
Rabee poſterior - -	29
Joomad prior - -	30
Joomad poſterior -	29
Rajab - - -	30
Saban - - -	29
Ramadan - - -	30
Sawal - - -	29
Dulkaidat - - -	30
Dulhajee - - -	30
Days	355

Ended 25 June 624.

It may not be unintereſting to examine the rule by which the Arabians appear to have been guided, in placing the intercalary day at the end of thoſe particular years which have been ſpecified. It was obſerved, that the annual exceſs is calculated to be 11 parts in 30 of a day. At the commencement of the firſt year of their firſt cycle, they appear to have aſſumed the fact (ſomewhat capriciouſly) that there was then an exceſs of 11 parts, belonging to the preceding year, to be accounted for, or brought on. At the end of the firſt year there would conſequently be 22 ſuch parts; and at the end of the ſecond year 33 parts. Here then the firſt intercalary day was applied; that ſecond year was made to conſiſt of 355 days, and there remained 3 parts, over and above, to be carried on to the next.

At the expiration of the third year, the parts amounted to 14; of the fourth year, to 25; and of the fifth, to 36; when the intercalation was again applied, and a balance of 6 parts carried on. From this it will be underſtood in what manner the fractional exceedings of each year were combined and diſpoſed of through the ſucceeding years of the cycle; and it will be neceſſary only further to remark, that, when the aggregate of the fractions falls ſhort no more than 2 or 3 parts of the number of 30, they ſtill add the intercalary day, and deduct the deficiency from the exceſs of the following year, which, in the courſe of one cycle, takes place only three times. At the end of the 29th year, the accumulated fractions, amounting exactly to 30, are commenſurate with the intercalation then applied; and the exceſs of the 30th, or laſt year, is accounted for in the firſt intercalation of the ſucceeding period. The operation would doubtleſs have appeared more methodical, if the firſt intercalary day were not to have been added till the end of the third year, and the eleventh, or laſt, till the end of the 30th year or termination of the cycle. From this conſideration ſome commentators have been led to diſſent from the more general idea, as above given, and to ſuggeſt, that the emboliſm is in fact applied ſo ſoon after the commencement of the cycle, as the yearly accumulation of the fractional parts exceeds the ſum of half a day, or twelve hours, and that it accordingly is made to take place at the end of the ſecond year, becauſe the fractions then amount to 17 h. 36′, or 22 parts in 30; at the end of the fifth year, becauſe they then amount to 25; and at the end of the ſeventh year, to 17 parts; keeping thus as near as poſſible to the mean diviſion of time, by applying the compenſation before it is fully wanted. The effect, however, is in both caſes the ſame, and it is of but little moment to determine which theory is right[*].

This cycle of thirty Mahometan years, contains 10,631 days, and is equal to 29 years and 39 days of our computation. The annual mean difference is 10 days and 21 hours nearly; which in common calculations, for ſhort periods of time, may be reckoned at 11 days, by which number the lunar year anticipates the ſolar.

Annexed hereto is a table exhibiting the correſpondence of the years of the Hejerà, from the eſtabliſhment of that epoch, with thoſe of the Chriſtian era, to the year of our Lord 2000. Until the beginning of the preſent century, it appears ſufficient to diſtinguiſh every tenth year; the intervals between which may be calculated with eaſe and preciſion, by attending to what has been ſaid reſpecting the cycle. From the year 1700 to the concluſion of the twentieth century, for the convenience of hiſtorians yet unborn, the commencement of each year of the Hejerà is aſcertained. Theſe tables are founded upon thoſe of GRAVIUS (J. GREAVES), in his Epochæ celebriores ULUG BEIGI, publiſhed in 1650; but as he, in conformity with the principles of this celebrated Tartarian aſtronomer [†], has fixed the epoch of the Hejerà to the 15th July,

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[*] The former hypotheſis is ſupported by CHRISTMANNUS (Commentarius ad caput primum ALFRAGANI, 1590), who quotes many ancient authorities. PETAVIUS (PETEAU) (de Doctrinà Temporum, 1627) is inclined to give the preference to the latter, on the authority of PAULUS FOROSEMPRONIUS, and the probability of the matter (*n*). It does not appear at what time the uſe of this cycle was introduced, but probably ſubſeqnent to the eſtabliſhment of the era by OMAR, though he is ſaid to have been aſſiſted on that occaſion by a learned Perſian.

[†] ULUG BEIG was the grandſon of TIMUR the Great (TAMERLANE), to whoſe empire he ſucceeded on the death of his father SHAH RUKH. He was born in 1393, and died in 1449.

inſtead of the 16th, or hiſtorical period, it was judged requiſite to add one day, throughout, to his calculations. The propriety of this alteration is ſtrengthened by the authority of other chronologiſts *, and by the practice of the modern almanacs †. It is alſo to be obſerved, that the tables of GRAVIUS, having been compoſed in the ſeventeenth century, are calculated both for paſt and future time, according to the old ſtyle; and as the change took place, in England, in September of the year 1752, it was neceſſary to adjuſt all the ſucceeding years to the new calendar. In order that a judgment may be formed of the correſpondence of the annual periods ſhewn by theſe tables, founded on the cycle of thirty years, which is adjuſted to the mean motion of the moon, with thoſe marked by the appearance of that planet, a ſhort table is ſubjoined, containing a compariſon of the reſults of the two modes of reckoning, during one cycle, commencing with the year of the Hejerà 1171 (*p*).

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* See Tables of the Hejerà in RICCIOLI, Chronologia Reformata, 1659. Ephemerides MAT. FRED. BECKII, 1695 (*o*).

† According to the original tables of GREAVES, the firſt day of Moharram, in the year of CHRIST 1783, falls on the 14th November, O.S., or 25th November, N.S.; and in 1784, on the 2d November, O.S., or 13th November, N.S.; whereas, by two almanacs, printed at Calcutta in Bengal, it appears, that the days ſhould be the 26th and 14th November. Of theſe almanacs, the one was compiled in the "Office of the Miſſion;" and the other by an ingenious aſtronomer from England; and both founded on the uſage of the Mahometans of India.

AUTHORITIES.

(*a*) "Epochæ verò hujus uſum introduxit OMAR, qui primus titulo hoc literas et diplomata ſignari juſſit; et quidem anno ejuſdem epochæ demum 18, menſe Gjumadâ poſteriore; ut ex Hiſtoriâ Sarac Ibn Amîdi, et aliunde liquet." GOLIUS, notæ in ALFERGANUM, 1669.

(*b*) "Antè illud tempus ſimilis quoque Arabibus mos fuerat putare ſuos annos à Bellorum initiis, et celebrioribus pugnis." GOLIUS ut ſuprà.

(*c*) "Arabes annos Higræ ſeu migrationis Muhammedicæ numerant à neomeniâ Muharram; licet Muhammed Meccâ exceſſerit die primo Rabiæ prioris, diebus 59 poſt epochæ ipſius diem." GOLIUS ut ſuprà.

(*d*) "Initium hujus epochæ eſt principium Moharram, illius anni in quo Propheta noſter MOHAMMADES à Meccâ ad Medinam migrabat; et illud ſecundùm medium calculum, eſt feria quinta (dies Jovis), ſed ſecundùm phaſim Lunæ, dies Veneris." ULUG BEIG, 1449, per GRAVIUM, 1650.

"Æra Arabum ducitur à principio ejus anni, quo Muhammed, relictâ Meccâ, commigravit Medinam: eratque iſtius anni initium feria quinta." ALFERGANUS, 950, per GOLIUM, 1669.

"Sequimur autem Saracenum, cùm facimus initium annorum Hegiræ à feriâ ſextâ: is enim ita ſcribit: 'Quidam incipiunt computationem menſium à nocte quintæ feriæ, et ponunt ibi Almuharam anni repulſionis: et quidam ponunt ipſum à nocte diei Veneris, et ſecundùm illud fixus eſt iſte noſter liber: quoniam iſte dies fuit initium anni veri, et eſt magis conveniens ortui novæ lunæ.' Ex quibus verbis perſpicere licet, nos Hegiram incipere à phaſi lunari, quæ incidit in principium feriæ ſextæ, et minimùm integro die poſt conjunctionem mediam ſolis et lunæ in oculos incurrit." CHRISTMANNUS, 1590.

(*e*) "At ſextus peculiariter dies Conventûs dicitur; quia eo, utpote ſibi ſacra, in templum cathedrale convenire ſolent." GOLIUS ut ſuprà.

"Commodùm igitur tunc evenit, ut in eundem diem feria ſexta, quæ Veneri conſecrata fuit, et neomenia popularis, ac luna corniculata, &c." PETAVIUS de Doctrina Temporum.

(*f*) "Cauſæ autem primam Lunæ viſionem vel retardantes vel promoventes, tres potiſſimùm in aſtronomiâ redduntur. Prima eſt obliquitas ſpheræ, longos vel breves occaſus adducens: in ſignis namque longarum deſcenſionum, nempe in Piſcibus, Ariete, et Tauro, fieri poteſt, ut prima phaſis Lunæ paulò poſt conjunctionem conſpiciatur. Altera cauſa eſt tempus conjunctionis Lunæ cum ſole circa limitem Boreum. Tertia denique, ſi luna ſit motu velox, qualis eſt circa Perigæum." HEVELII Selenographia, 1647.

(*g*) "Ita fit, ut primus illorum menſis, qui eſt Muharram, nullam certam in anno Juliano ſedem habeat; ſed quotannis antevertat; ac totum anni noſtri contextum peragret." PETAVIUS, Rationarium Temporum, 1702.

(*h*) "Mahomedani menſes hujus epochæ à phaſi novilunii ad phaſim ſequentis novilunii numerant. E duodecim menſibus annum conſtituunt. Anni, itaque, et menſes, ſecundùm eorum uſum, ſunt lunares veri." ULUG BEIG.

"Voluerant autem Arabes à veſperâ diem auſpicari; quoniam à phaſi lunari incipiunt numerare dies menſium." ALFRAGANUS, per CHRISTMANNUM.

"Omnes

" Omnes illæ gentes, quæ anno lunari ſunt uſæ, non ſolum hanc primam Lunæ apparitionem diligenter attenderunt, ſed etiam numerum principiumque cujuſlibet menſis ab eâ inierunt." HEVELIUS.

(*i*) " Menſes, ſecundùm PTOLEMÆUM, computantur ab unâ mediâ ſolis et lunæ conjunctione, uſque ad alteram: verus autem menſis incipit poſt digreſſum luminarium, elapſo uno die naturali." ALFRAGANUS, per CHRISTMANNUM.

(*k*) " Neque enim tempus quod à phaſi petitur, certò deſignari poteſt, hominibus præſertim longiùs diſſitis, aut poſt venturis; quod res civiles ac politicæ requirunt: aſtronomicæ verò et chronologicæ conſiſtere abſque eo neutiquam poſſunt. Sacræ, autem, quia juxta phaſin primùm inſtitutæ fuerant, juxta eandem quoque obſervari debuerunt." GOLIUS ut ſupra.

(*l*) " Satis intelligitur, voluiſſe Arabes de induſtriâ recedere à calendario Judaico, commodioremque annorum ſuorum periodem et rationem intercalandi obſervare." CHRISTMANNUS.

(*m*) " Aſtronomis, Moharram (menſis primus) triginta eſt dierum, et Safar (ſecundus) undetriginta, et ſimiliter menſium unus triginta dierum, et alius undetriginta uſque ad finem anni. Singulis autem annis tricenis, menſem Dulheggiah (poſtremum) undecies conſtituunt triginta dierum; atque hoc fit anno ſecundo, et quinto, &c." ULUG BEIG.

" Sunt autem menſes quidam 30, nonnulli 29 dierum; ut annus ſimplex conſtet 354 diebus. Menſes Arabum in phaſi lunari variant pro luminis additione vel diminutione: ideo rectè conſtitutum eſt, ut menſes alternatim ſint pleni et cavi. Spacio triginta annorum accreſcunt undecim dies intercalares: ratione veræ anni quantitatis, quæ ponitur 354 dierum et $\frac{11}{30}$ diei. Annus, in quo fractiones colliguntur, habet 7 menſes plenos et 5 cavos. Menſis cui adhibetur intercalatio ſemper eſt dierum 30; unde annus intercalaris nuncupatur, qui menſi

Dhilhaga (Dulhajee) diem adjicit, ut notum eſt omnibus." ALFRAGANUS, per CHRISTMANNUM.

(*n*) "Sed civili ratione tum dies unus intercalatur, cùm horariæ appendices dimidiatum diem ſuperant, ut Paulus exiſtimat. Alii ut CHRISTMANNUS, &c." PETAVIUS, Doctrina Temporum.

(*o*) "G. KIRCHIUS in calendario Chriſt°-Jud°-Turc°, præfati anni 1687 uno die ſeriùs dictum annum Turcarum recenſet." MAT. FRID. BECKIUS, Eph. 1695.

(*p*) "Non enim ſemper contingit, initium menſis, ratione, et calculi et primæ phaſeos, incidere in eundem diem: neque hoc fit, niſi tractu temporis ambo inter ſe æquentur." ALFERGANUS, per GOLIUM.

Table exhibiting the correſpondence of the years of the Hejerà with thoſe of the Chriſtian era.

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1	622	16 July	F	181	797	5 Mar.	Su	361	971	24 Oct.	Tu
11	632	29 Mar.	Su	191	806	17 Nov.	Tu	371	981	7 July	Th
21	641	10 Dec.	M	201	816	30 July	W	381	991	20 Mar.	F
31	651	24 Aug.	W	211	826	13 Apr.	F	391	1000	1 Dec.	Su
41	661	7 May	F	221	835	26 Dec.	Su	401	1010	15 Aug.	Tu
51	671	18 Jan.	Sa	231	845	7 Sept.	M	411	1020	27 Apr.	W
61	680	1 Oct.	M	241	855	22 May	W	421	1030	9 Jan.	F
71	690	15 June	W	251	865	2 Feb.	F	431	1039	23 Sept.	Su
81	700	26 Feb.	Th	261	874	16 Oct.	Sa	441	1049	5 June	M
91	709	9 Nov.	Sa	271	884	29 June	M	451	1059	17 Feb.	W
101	719	24 July	M	281	894	13 Mar.	W	461	1068	31 Oct.	F
111	729	5 Apr.	Tu	291	903	24 Nov.	Th	471	1078	14 July	Sa
121	738	18 Dec.	Th	301	913	7 Aug.	Sa	481	1088	27 Mar.	M
131	748	31 Aug.	Sa	311	923	21 Apr.	M	491	1097	9 Dec.	W
141	758	14 May	Su	321	933	1 Jan.	Tu	501	1107	22 Aug.	Th
151	768	26 Jan.	Tu	331	942	15 Sept.	Th	511	1117	5 May	Sa
161	777	9 Oct.	Th	341	952	29 May	Sa	521	1127	17 Jan.	M
171	787	22 June	F	351	962	9 Feb.	Su	531	1136	29 Sept.	Tu

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
541	1146	13 June	Th	971	1563	21 Aug.	Sa	1139	1726	18 Aug.	Th
551	1156	25 Feb.	Sa	981	1573	3 May	Su	1140	1727	8 Aug.	Tu
561	1165	7 Nov.	Su	991	1583	15 Jan.	Tu	1141	1728	27 July	Sa
571	1175	22 July	Tu	1001	1592	28 Sept.	Th	1142	1729	16 July	W
581	1185	4 Apr.	Th	1011	1602	11 June	F	1143	1730	6 July	M
591	1194	16 Dec.	F	1021	1612	23 Feb.	Su	1144	1731	25 June	F
601	1204	29 Aug.	Su	1031	1621	6 Nov.	Tu	1145	1732	13 June	Tu
611	1214	13 May	Tu	1041	1631	20 July	W	1146	1733	3 June	Su
621	1224	24 Jan.	W	1051	1641	2 Apr.	F	1147	1734	23 May	Th
631	1233	7 Oct.	F	1061	1650	15 Dec.	Su	1148	1735	13 May	Tu
641	1243	21 June	Su	1071	1660	27 Aug.	M	1149	1736	1 May	Sa
651	1253	3 Mar.	M	1081	1670	11 May	W	1150	1737	20 Apr.	W
661	1262	15 Nov.	W	1091	1680	23 Jan.	F	1151	1738	10 Apr.	M
671	1272	29 July	F	1101	1689	5 Oct.	Sa	1152	1739	30 Mar.	F
681	1282	11 Apr.	Sa	1111	1699	19 June	M	1153	1740	18 Mar.	Tu
691	1291	24 Dec.	M					1154	1741	8 Mar.	Su
701	1301	6 Sept.	W	1112	1700	7 June	F	1155	1742	25 Feb.	Th
711	1311	20 May	Th	1113	1701	28 May	W	1156	1743	15 Feb.	Tu
721	1321	31 Jan.	Sa	1114	1702	17 May	Su	1157	1744	4 Feb.	Sa
731	1330	15 Oct.	M	1115	1703	6 May	Th	1158	1745	23 Jan.	W
741	1340	27 June	Tu	1116	1704	25 Apr.	Tu	1159	1746	13 Jan.	M
751	1350	11 Mar.	Th	1117	1705	14 Apr.	Sa	1160	1747	2 Jan.	F
761	1359	23 Nov.	Sa	1118	1706	4 Apr.	Th	1161	1747	22 Dec.	Tu
771	1369	5 Aug.	Su	1119	1707	24 Mar.	M	1162	1748	11 Dec.	Su
781	1379	19 Apr.	Tu	1120	1708	12 Mar.	F	1163	1749	30 Nov.	Th
791	1388	31 Dec.	Th	1121	1709	2 Mar.	W	1164	1750	19 Nov.	M
801	1398	13 Sept.	F	1122	1710	19 Feb.	Su	1165	1751	9 Nov.	Sa
811	1408	27 May	Su	1123	1711	8 Feb.	Th	1166	1752	8 Nov.	W
821	1418	8 Feb.	Tu	1124	1712	29 Jan.	Tu	1167	1753	29 Oct.	M
831	1427	22 Oct.	W	1125	1713	17 Jan.	Sa	1168	1754	18 Oct	F
841	1437	5 July	F	1126	1714	7 Jan.	Th	1169	1755	7 Oct.	Tu
851	1447	19 Mar.	Su	1127	1714	27 Dec.	M	1170	1756	26 Sept.	Su
861	1456	3 Nov.	M	1128	1715	16 Dec.	F	1171	1757	15 Sept.	Th
871	1466	13 Aug.	W	1129	1716	5 Dec	W	1172	1758	4 Sept.	M
881	1476	26 Apr.	F	1130	1717	24 Nov.	Su	1173	1759	25 Aug.	Sa
891	1486	7 Jan.	Sa	1131	1718	13 Nov.	Th	1174	1760	13 Aug.	W
901	1495	21 Sept.	M	1132	1719	3 Nov.	Tu	1175	1761	2 Aug.	Su
911	1505	4 June	W	1133	1720	22 Oct.	Sa	1176	1762	23 July	F
921	1515	15 Feb.	Th	1134	1721	11 Oct	W	1177	1763	12 July	Tu
931	1524	29 Oct.	Sa	1135	1722	1 Oct.	M	1178	1764	1 July	Su
941	1534	13 July	M	1136	1723	20 Sept.	F	1179	1765	20 June	Th
951	1544	25 Mar.	Tu	1137	1724	9 Sept.	W	1180	1766	9 June	M
961	1553	7 Dec.	Th	1138	1725	29 Aug.	Su	1181	1767	30 May	Sa

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1182	1768	18 May	W	1225	1810	5 Feb.	Tu	1268	1851	26 Oct.	M
1183	1769	7 May	Su	1226	1811	25 Jan	Sa	1269	1852	14 Oct.	F
1184	1770	27 Apr.	F	1227	1812	15 Jan.	Th	1270	1853	3 Oct.	Tu
1185	1771	16 Apr.	Tu	1228	1813	3 Jan.	M	1271	1854	23 Sept.	Su
1186	1772	5 Apr.	Su	1229	1813	23 Dec.	F	1272	1855	12 Sept.	Th
1187	1773	25 Mar.	Th	1230	1814	13 Dec.	W	1273	1856	31 Aug.	M
1188	1774	14 Mar.	M	1231	1815	2 Dec.	Su	1274	1857	21 Aug.	Sa
1189	1775	4 Mar.	Sa	1232	1816	20 Nov.	Th	1275	1858	10 Aug.	W
1190	1776	21 Feb.	W	1233	1817	10 Nov.	Tu	1276	1859	31 July	M
1191	1777	9 Feb.	M	1234	1818	30 Oct.	Sa	1277	1860	19 July	F
1192	1778	30 Jan	F	1235	1819	19 Oct.	W	1278	1861	8 July	Tu
1193	1779	19 Jan.	Tu	1236	1820	8 Oct.	M	1279	1862	28 June	Su
1194	1780	8 Jan.	Sa	1237	1821	27 Sept.	F	1280	1863	17 June	Th
1195	1780	28 Dec.	Th	1238	1822	17 Sept.	W	1281	1864	5 June	M
1196	1781	17 Dec.	M	1239	1823	6 Sept.	Su	1282	1865	26 May	Sa
1197	1782	7 Dec.	Sa	1240	1824	25 Aug.	Th	1283	1866	15 May	W
1198	1783	26 Nov.	W	1241	1825	15 Aug.	Tu	1284	1867	4 May	Su
1199	1784	14 Nov.	Su	1242	1826	4 Aug.	Sa	1285	1868	23 Apr.	F
1200	1785	4 Nov.	F	1243	1827	24 July	W	1286	1869	12 Apr.	Tu
1201	1786	24 Oct.	Tu	1244	1828	13 July	M	1287	1870	2 Apr.	Su
1202	1787	13 Oct	Sa	1245	1829	2 July	F	1288	1871	22 Mar.	Th
1203	1788	2 Oct.	Th	1246	1830	22 June	W	1289	1872	10 Mar.	M
1204	1789	21 Sept.	M	1247	1831	11 June	Su	1290	1873	28 Feb.	Sa
1205	1790	10 Sept.	F	1248	1832	30 May	Th	1291	1874	17 Feb.	W
1206	1791	31 Aug.	W	1249	1833	20 May	Tu	1292	1875	6 Feb.	Su
1207	1792	19 Aug.	Su	1250	1834	9 May	Sa	1293	1876	27 Jan.	F
1208	1793	9 Aug.	F	1251	1835	28 Apr.	W	1294	1877	15 Jan.	Tu
1209	1794	29 July	Tu	1252	1836	17 Apr.	M	1295	1878	4 Jan.	Sa
1210	1795	18 July	Sa	1253	1837	6 Apr.	F	1296	1878	25 Dec.	Th
1211	1796	7 July	Th	1254	1838	26 Mar.	Tu	1297	1879	14 Dec.	M
1212	1797	26 June	M	1255	1839	16 Mar.	Su	1298	1880	3 Dec.	Sa
1213	1798	15 June	F	1256	1840	4 Mar.	Th	1299	1881	22 Nov.	W
1214	1799	5 June	W	1257	1841	22 Feb	Tu	1300	1882	11 Nov.	Su
1215	1800	24 May	Su	1258	1842	11 Feb.	Sa	1301	1883	1 Nov.	F
1216	1801	14 May	F	1259	1843	31 Jan.	W	1302	1884	20 Oct.	Tu
1217	1802	3 May	Tu	1260	1844	21 Jan.	M	1303	1885	9 Oct.	Sa
1218	1803	22 Apr.	M	1261	1845	9 Jan.	F	1304	1886	29 Sept.	Th
1219	1804	11 Apr.	Th	1262	1845	29 Dec.	Tu	1305	1887	18 Sept.	M
1220	1805	31 Mar.	M	1263	1846	19 Dec.	Su	1306	1888	7 Sept.	Sa
1221	1806	20 Mar.	F	1264	1847	8 Dec.	Th	1307	1889	27 Aug.	W
1222	1807	10 Mar.	W	1265	1848	26 Nov.	M	1308	1890	16 Aug.	Su
1223	1808	27 Feb.	Su	1266	1849	16 Nov.	Sa	1309	1891	6 Aug.	F
1224	1809	15 Feb.	Th	1267	1850	5 Nov.	W	1310	1892	25 July	Tu

An.

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1311	1893	14 July	Sa	1348	1929	7 June	Su	1385	1965	30 Apr.	Su
1312	1894	4 July	Th	1349	1930	27 May	Th	1386	1966	20 Apr.	F
1313	1895	23 June	M	1350	1931	17 May	Tu	1387	1967	9 Apr.	Tu
1314	1896	11 June	F	1351	1932	5 May	Sa	1388	1968	29 Mar.	Su
1315	1897	1 June	W	1352	1933	24 Apr	W	1389	1969	18 Mar.	Th
1316	1898	21 May	Su	1353	1934	14 Apr.	M	1390	1970	7 Mar.	M
1317	1899	11 May	F	1354	1935	3 Apr.	F	1391	1971	25 Feb.	Sa
1318	1900	29 Apr.	Tu	1355	1936	22 Mar.	Tu	1392	1972	14 Feb.	W
1319	1901	18 Apr.	Sa	1356	1937	12 Mar.	Su	1393	1973	2 Feb.	Su
1320	1902	8 Apr.	Th	1357	1938	1 Mar.	Th	1394	1974	23 Jan.	F
1321	1903	28 Mar.	M	1358	1939	19 Feb	Tu	1395	1975	12 Jan.	Tu
1322	1904	16 Mar.	F	1359	1940	8 Feb.	Sa	1396	1976	2 Jan.	Su
1323	1905	6 Mar.	W	1360	1941	27 Jan.	W	1397	1976	21 Dec.	Th
1324	1906	23 Feb.	Su	1361	1942	17 Jan.	M	1398	1977	10 Dec.	M
1325	1907	12 Feb.	Th	1362	1943	6 Jan.	F	1399	1978	30 Nov.	Sa
1326	1908	2 Feb.	Tu	1363	1943	26 Dec.	Tu	1400	1979	19 Nov.	W
1327	1909	21 Jan.	Sa	1364	1944	15 Dec.	Su	1401	1980	7 Nov.	Su
1328	1910	11 Jan.	Th	1365	1945	4 Dec.	Th	1402	1981	28 Oct.	F
1329	1910	31 Dec.	M	1366	1946	24 Nov.	Tu	1403	1982	17 Oct.	Tu
1330	1911	20 Dec.	F	1367	1947	13 Nov.	Sa	1404	1983	6 Oct.	Sa
1331	1912	9 Dec.	W	1368	1948	1 Nov.	W	1405	1984	25 Sept.	Th
1332	1913	28 Nov.	Su	1369	1949	22 Oct.	M	1406	1985	14 Sept.	M
1333	1914	17 Nov.	Th	1370	1950	11 Oct.	F	1407	1986	4 Sept.	Sa
1334	1915	7 Nov.	Tu	1371	1951	30 Sept.	Tu	1408	1987	24 Aug.	W
1335	1916	26 Oct.	Sa	1372	1952	19 Sept.	Su	1409	1988	12 Aug.	Su
1336	1917	16 Oct.	Th	1373	1953	8 Sept.	Th	1410	1989	2 Aug.	F
1337	1918	5 Oct.	M	1374	1954	28 Aug.	M	1411	1990	22 July	Tu
1338	1919	24 Sept.	F	1375	1955	18 Aug.	Sa	1412	1991	11 July	Sa
1339	1920	13 Sept.	W	1376	1956	6 Aug.	W	1413	1992	30 June	Th
1340	1921	2 Sept.	Su	1377	1957	27 July	M	1414	1993	19 June	M
1341	1922	22 Aug.	Th	1378	1958	16 July	F	1415	1994	8 June	F
1342	1923	12 Aug.	Tu	1379	1959	5 July	Tu	1416	1995	29 May	W
1343	1924	31 July	Sa	1380	1960	24 June	Su	1417	1996	17 May	Su
1344	1925	20 July	W	1381	1961	13 June	Th	1418	1997	7 May	F
1345	1926	10 July	M	1382	1962	2 June	M	1419	1998	26 Apr.	Tu
1346	1927	29 June	F	1383	1963	23 May	Sa	1420	1999	15 Apr.	Sa
1347	1928	18 June	W	1384	1964	11 May	W	1421	2000	4 Apr.	Th

Compariſon of the commencement of thirty ſucceſſive years, according to the diviſion of the cycle, and the appearance of the new moon.

	A. H.	A. C.	Cycle.	Conjunction.		☽ viſible.
				Day.	Hour.	
1	1171	1757	15 Sept.	13 Sept.	14	15 Sept.
2	– 1172	1758	4 Sept.	2 Sept.	15	4 Sept.
3	1173	1759	25 Aug,	22 Aug.	16	24 Aug.
4	1174	– 1760	13 Aug.	11 Aug.	1	12 Aug.
5	– 1175	1761	2 Aug.	31 July	15	2 Aug.
6	1176	1762	23 July	21 July	9	23 July
7	– 1177	1763	12 July	10 July	23	12 July
8	1178	– 1764	1 July	29 June	8	30 June
9	1179	1765	20 June	18 June	11	20 June.
10	– 1180	1766	9 June	7 June	12	9 June
11	1181	1767	30 May	27 May	19	29 May
12	1182	– 1768	18 May	16 May	8	17 May
13	– 1183	1769	7 May	6 May	1	7 May
14	1184	1770	27 April	25 April	17	27 April
15	– 1185	1771	16 April	15 April	3	16 April
16	1186	– 1772	5 April	3 April	5	4 April
17	1187	1773	25 Mar.	23 Mar.	5	24 Mar.
18	– 1188	1774	14 Mar.	12 Mar.	10	14 Mar.
19	1189	1775	4 Mar.	1 Mar.	22	3 Mar.
20	1190	– 1776	21 Feb.	19 Feb.	13	21 Feb.
21	– 1191	1777	9 Feb.	8 Feb.	5	9 Feb.
22	1192	1778	30 Jan.	28 Jan.	15	30 Jan.
23	1193	1779	19 Jan.	17 Jan.	18	19 Jan.
24	– 1194	– 1780	8 Jan.	6 Jan.	17	8 Jan.
25	1195	1780	28 Dec.	25 Dec.	20	27 Dec.
26	– 1196	1781	17 Dec.	15 Dec.	6	16 Dec
27	1197	1782	7 Dec.	4 Dec.	21	6 Dec.
28	1198	1783	26 Nov.	24 Nov.	13	26 Nov.
29	1199	– 1784	14 Nov.	13 Nov.	1	14 Nov.
30	1200	1785	4 Nov.	2 Nov.	4	3 Nov.

The years marked thus – are the intercalated and biſſextile years in the Mahometan and Roman calendars.

PRESENTS

MADE TO THE

ROYAL SOCIETY

From November 1787 to June 1788;

WITH

The NAMES of the DONORS.

	Preſents.	Donors.
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